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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

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of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1799, the Medal on Sir GODFREY COPLEY's Donation, to the Rev. JOHN HELLINS, B.D. F.R.S. for his improved Solution of a Problem in physical Astronomy, *&c.* printed in the Philosophical Transactions for the year 1798, and his other mathematical papers.

PHILOSOPHICAL TRANSACTIONS.

1. *The Croonian Lecture. On the Structure and Uses of the Membrana Tympani of the Ear.* By Everard Home, *Esq. F. R. S.*

Read November 7, 1799.

The subject of inquiry appointed by the Croonian Institution, has been greatly elucidated at different times by ingenious members of this learned Society. A large field, however, still remains open; and, respecting future investigations, I shall have occasion to offer a fresh proof of the aid to be derived from comparative anatomy, in ascertaining the structure of parts which, from their minuteness and situation in the human body, admit with much difficulty of being explored.

The principal object of the present lecture is to communicate a discovery of the structure of the membrana tympani; which, in some respects, affords a new and very curious instance of the application of muscular action, and may conduce to account for certain phænomena in the sense of hearing, in a more satisfactory manner than has hitherto been proposed.

The membrana tympani has always been considered as a

common membrane, which, by means of muscles belonging to the malleus being stretched or relaxed, became fitted, in its various degrees of tension, to convey the vast variety of external sounds to the internal organ. Its shape, situation, and office, have procured it the name of drum of the ear; and the muscles of the malleus having been deemed sufficient for bracing and unbracing it, less attention was bestowed on the structure of the membrane itself: to which may be added, that in the human ear, and generally in the ear of quadrupeds, the membrane is so extremely small and thin, and in its situation so peculiarly confined, as not to be got at for inspection but with much difficulty.

The case is different in the elephant, where this membrane is so very large, that the parts of which it is composed are readily distinguished: they are even conspicuous to the naked eye; and muscular fibres are seen passing along the membrane, in a radiated manner, from the bony rim which surrounds it, towards the handle of the malleus, to which the central part of the membrane is firmly attached.

This discovery in the elephant having led to that of a similar construction in the human membrana tympani, it may not be improper to relate the circumstances by which I became engaged in the investigation of the organ of hearing in that animal.

Three different opportunities have occurred of dissecting the elephant in London, by the deaths of those which had been presented to his Majesty, and were kept at the King's stables at Pimlico. One of them was given to the late Dr. HUNTER; one to his brother Mr. J. HUNTER; and the third to Sir ASHTON LEVER.

From my being connected with Mr. JOHN HUNTER's pursuits in comparative anatomy, I was employed throughout the whole of these dissections, and became extremely desirous of examining the internal parts of the ear, the structure of that organ in the human body having at a very early period particularly engaged my attention;* but neither Dr. HUNTER nor his brother could be prevailed upon to sacrifice so large a portion of the skull as was necessary for the purpose.

When Mr. CORSE arrived from Bengal, last year, and mentioned his having brought over a number of skulls of elephants, in order to show the progress of the formation of their grinding teeth,† the desire to examine the organ of hearing in that animal recurred to me so strongly, that I requested to have one of the skulls for that purpose, and Mr. CORSE very readily and obligingly complied with my request.

After having examined the organ in the dried skull, the want of the membrana tympani, and of the small bones, made the information thus received of a very unsatisfactory nature, and increased the desire of seeing these parts in the recent head. In considering how this could be done, I recollected a mutilated elephant's head, preserved in spirits, which had been sent to

* In the year 1776, I injected the cochlea and semicircular canals of the human ear with a composition of wax and rosin. This was done by placing the temporal bone in the receiver of an air pump, the upper part of which was in the form of a funnel, rendered air-tight by a cork being fitted into its neck, and surrounded with bees' wax. After the air had been exhausted, the hot injection, poured into the funnel, melted the wax, and the cork was pulled out by means of a string previously attached to it; the injection immediately rushed into the receiver, and was forced, by the pressure of the atmosphere, into the cavities of the temporal bone.

† On this subject, a very ingenious paper has been since published by him, in the Philosophical Transactions for the year 1799.

Mr. HUNTER, but, from the multiplicity of his engagements, had remained neglected in the cask at the time of his death, and in the following year was dried, to show the proboscis, that it might not be altogether spoiled.

Upon examining this dried head, the bones had been so much broken, that one of the organs of hearing was altogether wanting: the other, however, was fortunately entire; and the membrana tympani and small bones, having been little disturbed in the drying of the parts, remained nearly in their natural situation.

The membrana tympani, and every other part of the organ, were found to be much larger in proportion than in other quadrupeds, or in man; differing in this respect from the eye of the elephant, which is unusually small, when compared with the enormous bulk of the animal.

The membrane was found of an oval form; the short diameter of the oval rather more than an inch in length; the long diameter an inch and $\frac{7}{20}$ths.

In the human ear, the membrana tympani is nearly circular; the longest diameter is $\frac{4}{20}$ths of an inch; the shortest $\frac{7}{20}$ths.

As the membrane in the elephant exceeds that of the human ear in thickness as much as in extent, which is as the squares of their diameters, or in the proportion of 135 to 14, it is natural to conclude that the muscular fibres which are to stretch the one, must greatly exceed in strength those capable of producing the same degree of tension in the other.

From this statement, the muscular structure in the human membrana tympani will necessarily be so much less distinct than in the elephant, as scarcely to be visible to the naked eye, and will easily be overlooked by the most attentive observer, who is not directed by some previous information to examine

it under the most favourable circumstances; but, when these are attended to, it can be perceived without the aid of glasses.

If the membrana tympani of the human ear is completely exposed on both sides, by removing the contiguous parts, and the cuticular covering is carefully washed off from its external surface, then, by placing it in a clear light, the radiated direction of its fibres may be easily detected. If a common magnifying glass is used, they are rendered nearly as distinct as those of the elephant appear to the naked eye; their course is exactly the same; and they differ in nothing but in being formed upon a smaller scale.

When viewed in a microscope magnifying 23 times, the muscular fibres are beautifully conspicuous, and appear uniformly the same throughout the whole surface, there being no central tendons, as in the diaphragm; the muscular fibres appear only to form the internal layer of the membrane, and are most distinctly seen when viewed on that side.

In examining this membrane in different subjects, the parts were frequently found in a more or less morbid state. In one instance, the membrane was found loaded with blood-vessels, was less transparent than usual, and was united by close adhesion to the point of the long process of the incus. In another instance, there was a preternatural ossification adhering to it, at a small distance from the end of the handle of the malleus.

As muscles in general are supplied with blood-vessels in proportion to the frequency of their action, it is an object of importance to determine the vascularity of the membrana tympani. Upon this subject, my own want of information has been amply supplied by Dr. BAILLIE, who, in a communication upon this subject, showed me a preparation of the membrane, in which

the vessels had been most successfully injected with coloured wax.

In this preparation, the most beautiful of the kind I ever saw, the vessels in their distribution resembled those of the iris, and were nearly half as numerous: they anastomosed with one another in a similar manner, and their general direction was from the circumference to the handle of the malleus; from near this handle, a small trunk sent off branches, in a radiated manner, which anastomosed with those which had an opposite course.

This correspondence, in the number and distribution of blood-vessels, between the membrana tympani and the iris, is a strong circumstance in confirmation of that membrane being endowed with muscular action.

In the horse, the membrana tympani is smaller than in man; its long diameter is $\frac{3}{20}$ths of an inch; the short one $\frac{6}{20}$ths; and it is almost quite flat, while in man it is concave, which makes the difference of extent considerably exceed the difference in the diameters. In the horse, the fibrous structure is not visible to the naked eye; it is even indistinctly seen when viewed through a common magnifying glass; but in a microscope it is very visible, and in every other respect agrees in structure with the membrane in the human ear, and in that of the elephant.

In birds, the membrana tympani is larger in proportion than in the quadruped, and more circular in its shape.

In the goose, it is $\frac{6}{20}$ths of an inch in its longest diameter, and $\frac{5}{20}$ths in its shortest diameter. In the turkey, $\frac{7}{20}$ths by $\frac{5}{20}$ths. It is thinner in its coats in birds than in the horse, and to the naked eye has no appearance of fibres; but, when viewed in a microscope, there is a visible radiated structure, not very unlike the wire marks upon common writing paper.

In a former Lecture upon the Structure of Muscles,* in which a comprehensive view was taken of the subject, it was stated, that the organization necessary for muscular contraction could exist in an apparent membrane, and that a fasciculated structure was only necessary when muscular action was to be enabled to overcome resistance. The coats of the Tænia hydatigena were mentioned as an instance of the first; and the human heart as the most complex of the second. In comparing the membranæ tympani of different animals, they afford a beautiful illustration of the truth of this position.

In birds, where from the smallness of its size the resistance is very trifling, the membrane is very similar to the coat of an hydatid, only still thinner. In the elephant, fibres forming fasciculi are very distinct. The membrane of the horse, and that of the human ear, form the intermediate gradations.

The knowledge of a muscular structure in the membrana tympani, enables us to explain many phænomena in hearing, which have not hitherto been accounted for in a satisfactory manner. It is principally by means of this muscle that accurate perceptions of sound are communicated to the internal organ, and that the membrana tympani is enabled to vary the state of its tension, so as to receive them in the quick succession in which they are conveyed to it.

In the human ear, and in that of birds, the radiated fibres of the membrana tympani have their principal attachment to the extremity of the handle of the malleus, which is nearly in the centre of the membrane.

In the membrane of the elephant, which is oval, the attachment to the handle of the malleus is at some distance from the

* Philosophical Transactions for the year 1795.

centre. In the horse, deer, and cat, which have the membrane still more oval than the elephant, the handle of the malleus is situated in the long axis of the membrane, with its extremity extending beyond the centre, reaching nearer to the circumference; and the fibres of the radiated muscle are not only attached to its end, but also laterally to nearly the whole length of its handle.

This oval form of the membrana tympani, in those quadrupeds, and the very extensive attachment of the fibres of the radiated muscle to the handle of the malleus, may be the reason why their ears are not equally fitted to hear inarticulate sounds, as the ears of birds and of man.

Should this radiated muscle of the membrana tympani (which is probably the smallest in the body that has a distinct action) be thought too insignificant to have an office of so much consequence assigned to it, let it be remembered, that the size of muscles is no indication of their importance, but only of the resistance to be overcome by their action; and that the more delicate actions are performed universally in the body by very small muscles, of which the iris in the eye furnishes a very conspicuous example.

Before the mode in which the radiated muscle adapts the membrana tympani to different sounds can be explained, it is necessary that the more important parts of the organ should be enumerated, and the use commonly assigned to each of them pointed out.

In man and the more perfect quadrupeds, this organ consists of the following parts: the membrana tympani, situated between the external passage and the cavity of the tympanum; four small bones, which form a chain across the tympanum,

connecting the membrana tympani with another membrane lining the foramen ovale, which opens into the vestibulum, a more internal part of the organ of hearing.

The bones are, the malleus, which is united to the membrana tympani by a portion of its handle, and to the second bone or incus by its head. The incus, which is connected to the malleus by a capsular ligament, forming a regular joint, the surfaces of the bones being covered with cartilage, but they have only a tremulous motion on one another. The incus is also attached to the side of the cavity of the tympanum, where the mastoid cells open, by a ligament on which it moves backwards and forwards: it is united by its long process to the orbicular bone, which is the smallest in the body, and connects the incus to the fourth bone or stapes, which has its base applied to the foramen ovale, or opening leading into the cavity of the vestibulum.

The cavity of the tympanum, in which these bones are situated, communicates with the external air by means of the Eustachian tube, so that there is always air behind the membrana tympani.

The malleus has three muscles, by which it is moved; one of them is called the tensor, from its pulling the malleus inwards, and tightening the membrana tympani: the other two act in an opposite direction, and relax the membrane; the largest of these is called the obliquus, and is the antagonist of the tensor muscle; the other is very small, and is called the laxator.

The stapes has one muscle, which acts upon it by bringing its basis closer to the foramen ovale.

The vestibulum, which is completely separated from the tympanum, by the membrane that lines the foramen ovale, com-

municates freely with the cochlea and semicircular canals; but these cavities are filled with a watery liquor, and have no communication (as the tympanum has) with the external air.

This fact was ascertained in the horse, by the following experiment, repeated several times. The organ of hearing was separated from the skull immediately after death, and the cavity of the tympanum exposed; the parts were then immersed in water, and the stapes removed; by which means, the membrane of the foramen ovale was destroyed, but no globule of air was seen to escape through the water.*

The following uses have generally been assigned to the parts now mentioned.

The membrana tympani was supposed to be adapted to receive impressions, by the combined action of the tensor and laxator muscles varying the degree of its tension, so as to bring it in unison with different sounds: these impressions were conducted, by the chain of bones, to the vestibulum, cochlea, and semicircular canals; in which cavities, particularly the cochlea, they were supposed to undergo some modification, before they were impressed upon the nerves spread upon the linings of these cavities.

The function of modifying impressions of sound was assigned to the cochlea, partly from the delicacy of its internal structure, supposed to resemble a musical instrument, and partly from there being no other part of the organ apparently suited for repeating the variety of delicate sounds which pass into the ear: the changes that could be produced upon the membrana

* This experiment was made by Mr. CLIFT, who superintends Mr. HUNTER'S collection, and who has afforded me material assistance in the different parts of this investigation.

tympani by the muscles of the malleus, being considered as incapable of answering that purpose.

This slight sketch of the organ of hearing, and of the uses, as they are generally understood, of the different parts, will enable me to point out, with more clearness, what parts of the theory appear defective, and what improvements may be made on it.

It is true that the membrana tympani is stretched and relaxed by the action of the muscles of the malleus, but not for the purpose alleged in the commonly received theory. It is stretched, in order to bring the radiated muscle of the membrane itself into a state capable of acting, and of giving those different degrees of tension to the membrane which empower it to correspond with the variety of external tremors: when the membrane is relaxed, the radiated muscle cannot act with any effect, and external tremors make less accurate impressions.

The membrana tympani, with its tensor and radiated muscles, may be not unaptly compared to a monochord, of which the membrana tympani is the string; the tensor muscle the screw, giving the necessary tension to make the string perform its proper scale of vibrations; and the radiated muscle acting upon the membrane like the moveable bridge of the monochord, adjusting it to the vibrations required to be produced. The combined effects of the action of these muscles give the perceptions of grave and acute tones; and, in proportion as their original conformation is more or less perfect, so will their actions be, and, consequently, the perceptions of sound which they communicate.

This mode of subdividing the motions of the membrana tympani between two sets of muscles, allotting a portion to

each, is not peculiar to this part. A remarkable instance of it appears in the rapid movements of the fingers, in performing several actions, and particularly in playing on a musical instrument. In all such rapid motions, the fingers are bent to a certain degree by the long muscles that lie upon the fore-arm, to the tendons of which a set of smaller muscles are attached, called lumbricales. These last are unable to produce any effect on the fingers, till elongated in consequence of the action of the long muscles in bending the other joints; the lumbricales then become capable of bending the fingers a little more, and of acting with great rapidity. It is a curious circumstance, that a similar application of muscles should be employed to fit the fingers to produce a quick succession of sounds, and to enable the ear to be impressed by them.

From the explanation given of the adjustment of the membrana tympani, the difference between a musical ear and one which is too imperfect to distinguish the different notes in music, will appear to arise entirely from the greater or less nicety with which the muscle of the malleus renders the membrane capable of being truly adjusted. If the tension be perfect, all the variations produced by the action of the radiated muscle will be equally correct, and the ear truly musical; but, if the first adjustment is imperfect, although the actions of the radiated muscle may still produce infinite variations, none of them will be correct: the effect, in this respect, will be similar to that produced by playing upon a musical instrument which is not in tune. The hearing of articulate sounds requires less nicety in the adjustment, than of inarticulate or musical ones; an ear may therefore be able to perceive the one, although it is not fitted to receive distinct perceptions from the other.

The nicety or correctness of a musical ear being the result of muscular action, renders it, in part, an acquirement; for, although the original formation of these muscles in some ears renders them more capable of arriving at this perfection in their action, early cultivation is still necessary for that purpose; and it is found that an ear, which upon the first trials seemed unfit to receive accurate perceptions of sounds, shall, by early and constant application, be rendered tolerably correct, but never can attain excellence. There are organs of hearing in which the parts are so nicely adjusted to one another, as to render them capable of a degree of correctness in hearing sounds which appears preternatural.

Children who during their infancy are much in the society of musical performers, will be naturally induced to attend more to inarticulate sounds than articulate ones, and by these means acquire a correct ear, which, after listening for two or three years to articulate sounds only, would have been attained with more difficulty.

This mode of adapting the ear to different sounds, appears to be one of the most beautiful applications of muscles in the body; the mechanism is so simple, and the variety of effects so great.

Several ways in which the correctness of hearing is affected by the wrong actions of the muscles of the tympanum, that appeared to be inexplicable, can be readily accounted for, now that the means by which the membrane adjusts itself are understood. The following are instances of this kind.

Case 1. A gentleman thirty-three years of age, who possessed a very correct ear, so as to be capable of singing in concert, though he had never learned music, was suddenly seized with a

giddiness in the head, and a slight degree of numbness in the right side and arm. These feelings went off in a few hours, but on the third day returned, and for several weeks he had returns of the same sensations. It was soon discovered that he had lost his musical ear; he could neither sing a note in tune, nor in the smallest degree perceive harmony in the performance of others. For some time he himself thought he had become a little deaf, but his medical attendant was not sensible of that in conversation. Upon going into the country, he derived great benefit from exercise and sea-bathing.

Twenty months after the first attack, he was capable of singing a Scotch air with tolerable exactness, though he could not sing in concert. He continued to improve in his health, and in the course of two or three years completely recovered his ear for music.

In this case, there appeared to be some affection of the brain, which had diminished the actions of the tensor muscles of the membranæ tympani, through the medium of the nerves which regulate their actions; this gradually went off, and the muscles recovered their former action.

CASE 2. A young lady was seized with a frenzy which lasted for several years. Previous to her derangement, she was incapable of singing in tune, from the want of an ear for music; but in the course of her madness she frequently, to the astonishment of her relations, sung a tune with tolerable correctness.

This case is the reverse of the former; and, as it arose from a directly contrary affection of the brain, may be considered as the result of an unusual degree of action in the tensor muscles, giving the membrane a more correct adjustment than it had before.

Case 3. An eminent music master, after catching cold, found a confusion of sounds in his ears. Upon strict attention, he discovered that the pitch of one ear was half a note lower than that of the other; and that the perception of a simple sound did not reach both ears at the same instant, but seemed as two distinct sounds, following each other in quick succession, the last being the lowest and weakest. This complaint distressed him for a long time, but he recovered from it without any medical aid.

In this case, the whole defect appears to have been in the action of the radiated muscle, exerted neither with the same quickness nor force in one ear as in the other, so that the sound was half a note too low, as well as later in being impressed upon the organ.

This affection of the muscle of the membrana tympani is very similar to an affection of the straight muscles of one of the eyes, producing double vision, which I have noticed in a former lecture, when treating of the wrong actions of that organ.*

In endeavouring to explain the uses of the more internal parts of the ear, considerable advantage may be derived from classing them in two divisions; namely, those which are formed for the purpose of receiving impressions conveyed through the medium of liquid or of solid substances; and those adapted to receive impressions made by the impulses of an elastic fluid, as the common air.

This can be done very correctly. Fish, which are formed to hear in water, can have only the parts belonging to the first division; while all the parts found in the ears of birds and

* Vide Philosophical Transactions for the year 1797.

quadrupeds, that are not met with in fish, must belong to the second.

In fish, the organ consists of a vestibulum and three semicircular canals, and these are met with in all fish. In some genera there is an external opening, and substances of a hard nature are found lying loose in the vestibulum: these, however, cannot be considered as essential parts of the organ, from their not being common to fish in general.

Birds have the vestibulum and semicircular canals in common with fish, but they have also a membrana tympani; a slender bone connecting that membrane with the vestibulum; and an Eustachian tube. In birds, the membrana tympani is convex externally, being pushed forwards by the end of the slender bone abovementioned.

In quadrupeds and man, besides the vestibulum and canals met with in fish, the membrana tympani, the bone connecting it with the vestibulum, and the Eustachian tube, found in birds, there is a cochlea. The membrana tympani is either flat or concave externally; the bony connection between it and the vestibulum is made up of several bones, supplied with muscles to move them in different directions.

The parts which compose the organ of hearing in fish, must be intended for receiving impressions conveyed through water: those additional parts met with in birds, and the still greater additions which are found in the quadruped and man, must be intended by nature for rendering more perfect the impressions conveyed to the ear through the medium of the external air.

Fish, from the structure of the organ, can only hear sounds which agitate the water immediately in contact with the head of the fish; so that the impulse is conveyed, without inter-

ruption, from the liquid in which they live, to the organ of hearing.

Man is capable of hearing in a similar manner to fishes, when a communication of solid parts is kept up between the sounding body and the bones of the skull: experiments of this kind must have been made by many members of this learned Society.

One of the most common is, applying a watch to the forehead, and stopping the ears, which does not prevent the ticking from being heard: the sound is still more distinct when the watch is applied to the mastoid process. Here, as the sound can neither pass through the meatus externus, nor by the Eustachian tube, while the mouth is kept shut, it evidently must be conducted through the bones of the skull.

When the sound produced by boiling water is brought to the ear, by one end of an iron rod resting upon the side of the kettle and the other kept in contact with the teeth, the sound is conducted in the same way, although in this case it has by some been supposed to pass through the Eustachian tube.

In this mode of hearing, the vestibulum and semicircular canals are probably the only parts of the organ which are necessary to convey the impression to the expansion of the auditory nerve.

In hearing in air, the use of the membrana tympani in man and quadrupeds has already been explained. Its office in birds is precisely the same; but as in birds this membrane has no tensor muscle to vary its adjustment, but is always kept tense by the pressure of the end of the slender bone, the scale in birds cannot descend so low as in the human ear; and the intervals in their scale will be more minute, in consequence of the slightest tremor communicated by the action of the radiated

muscle to one end of the slender bone being immediately conducted to the internal organ; while in the human ear it has to pass from one bone to another, before it arrives at the vestibulum.

The cochlea has been considered by all physiologists as one of the most intricate and curious parts of the ear, and on that account had a most important office assigned to it. This, however, is now to be transferred to the membrana tympani; and, upon attentive consideration of the subject, it will appear impossible for the cochlea to be of any use in modulating sounds, since the ear is only intended to convey impressions received from external bodies; hence, no impression can be communicated to the cochlea, which has not been transmitted by the membrana tympani. But, if all the varieties of sound are repeated by the membrana tympani, no modulation in the cochlea is required; and, when it is considered that the cochlea contains water, instead of air, the effect upon every part will be found to be simultaneous.

That the cochlea is neither absolutely necessary to fit the organ to be impressed by sounds communicated through air, nor to render it what is termed a musical ear, is sufficiently proved by that part being wanting in birds, whose organ is particularly adapted to inarticulate sounds. Some birds, particularly bulfinches, can be taught to sing various airs, although it will be always in high notes.

If it should be found that birds hear less accurately than quadrupeds, it will favour the idea that the great delicacy of structure of the cochlea, is intended to render the nerves which are spread upon it more readily impressed by weak tremors, than those in either the vestibulum or semicircular canals.

The cochlea and semicircular canals must be considered as two of the most important parts of the ear; their peculiar forms are no doubt adapted to some essential purposes; but, what are the precise advantages derived from their particular shape, is at present unknown. There is, however, much ground to believe, that a more extensive knowledge in comparative anatomy, joined with future observations, may clear up this very curious and obscure part of the physiology of the organ of hearing.

In the elephant, the small bones, the cochlea, and semicircular canals, are larger than those in the human ear, nearly in the same proportion with the increased size of the membrana tympani. In that animal, there is a very remarkable peculiarity; which is, a cellular structure occupying the upper and posterior part of the skull, inclosed between the two tables, communicating by a considerable aperture with the cavity of the tympanum, and lined by a similar membrane: the cells communicate freely with one another at their lower extremities, but not near the upper, forming irregular cylinders, placed in a converging direction, towards the cavity of the tympanum.

There is no middle bony septum, separating the cells of the skull belonging to one ear from those which open into the other, but a ready communication between them.

On the anterior part of the skull there is also a similar cellular structure, only much smaller, which communicates with the nose, but is entirely separate and distinct from that which forms an appendage to the organ of hearing.

That the elephant hears better than other animals, is generally asserted by those who have had opportunities of making observations on the subject. As this opinion has been advanced by men who had no knowledge in anatomy, and had

no previous theory to bias their judgment, it is deserving of credit. The organ of hearing being now found more perfect, and formed upon a larger scale than in any other animal with which we are acquainted, considerable weight is given to this opinion.

Mr. CORSE, who resided many years at Tiperah, in Bengal, and paid particular attention to the manners and habits of elephants, concurs in the belief of their hearing being more acute than that of man. The following circumstances are mentioned by him in proof of it.

A tame elephant, who was never reconciled to have a horse moving behind him, although he expressed no uneasiness if the horse was within his view, either before or on one side, could distinguish the sound of a horse's foot at a distance, some time before any person in company heard it: this was known by his pricking up his ears, quickening his pace, and turning his head from side to side.

A tame female elephant, who had a young one, was occasionally sent out with other elephants for food, without the young one being allowed to follow. She was not in the habit of pining after her young one, unless she heard its voice; but frequently, on the road home, when no one could distinguish any sound whatever, she pricked up her ears, and made a noise expressive of having heard the call of her young. This having occurred frequently, attracted Mr. CORSE's notice, and made him, at the time the female elephant used these expressions, stop the party, and desire the gentlemen to listen; but they were unable to hear any thing till they had approached nearer to the place where the young one was kept.

The foregoing observations, the object of which has been to

prove that the membrana tympani of the ear has a muscular structure, have already exceeded the limits of a lecture, which prevents us from going further at present into the consideration of this very curious and important organ. The general analogy between the uses of its different parts and those of the organ of vision, and the similar variations of their actions when under the influence of disease, furnish materials which, on some future occasion, may be laid before this learned Society.

II. *On the Method of determining, from the real Probabilities of Life, the Values of Contingent Reversions in which three Lives are involved in the Survivorship.* By William Morgan, *Esq. F. R. S.*

Read December 12, 1799.

ON SURVIVORSHIPS.

THE several papers which I have had the honour of communicating to the Royal Society, on the doctrine of contingent reversions, contain the greater number of those cases in which three lives are concerned in the survivorship. With the view of completing this subject, I have been induced to investigate the remaining problems; and, having succeeded in the solution of them, I hope the following will not be considered as an improper addition to my former communications.

Being anxious to render this paper as concise as possible, I have omitted to state at length the different contingencies on which the payment of the given sum depends; trusting that, from an attentive perusal of my former demonstrations, these will appear to be so plainly expressed by the several fractions in each problem, as to render a more ample description of them unnecessary.

PROBLEM I.

To determine the value of a given sum, payable on the death

of A or B, should either of them be the *first* or *second* that fails, of the three lives, A, B, and C.

Solution.

In this case, the payment of the given sum must certainly take place on the extinction of the joint lives of A and B, independent of C, and therefore the value of the reversion will be $= \frac{S \cdot \overline{r-1} \cdot \overline{V - AB}}{r}$ *.

The fractions expressing the contingencies on which the payment of S depends, in the 1st year, are $\frac{S}{abcr} \times a' \cdot \overline{b-m} \cdot \overline{c-d} + a' \cdot \overline{b-m} \cdot d + a' \cdot \overline{c-d} \cdot m + \overline{b-m} \cdot \overline{c-d} \cdot \overline{a-a'} = \frac{S}{abcr} \times \overline{abc - mc \cdot \overline{a-a'}}$; in the 2d year $= \frac{S}{abcr^2} \times a'' \cdot \overline{m-n} \cdot \overline{\overline{d-e} + a'' \cdot \overline{m-n} \cdot e + a'' \times \overline{d-e} \cdot n + a'' \cdot ne + \overline{m-n} \cdot \overline{d-e} \cdot \overline{a-a'} + a''} + \overline{m-n} \cdot \overline{a-a' + a''} + \overline{m-n} \cdot \overline{a-a' + a''} \cdot e + \overline{c-d} \cdot n \cdot a'' + \overline{c-d} \cdot \overline{m-n} \cdot \overline{a-a' + a''} + \overline{c-d} \cdot \overline{m-n} \cdot a'' = \frac{S}{abcr^2} \times \overline{mc \cdot \overline{a-a} - ne \cdot \overline{a-a' + a''}}$, and so on in the other years; hence the whole value is $= \frac{S}{r} \times \overline{r-1} \cdot V - r \cdot AB + AB = \frac{S \cdot \overline{r-1}}{r} \cdot \overline{V - AB}$, as before. Q. E. D.

PROBLEM II.

To determine the value of a given sum, payable on the decease of A or B, should either of them be the *second* or *third* that shall fail, of the three lives, A, B, and C.

* The same symbols are uniformly retained in this, as in my last two papers on the subject. See Phil. Trans. Vol. LXXXI. page 247.

Solution.

In the 1st year, the value of the reversion will be $\frac{S}{abcr}$ into $a'.\overline{b-m}.\overline{c-d}+a.\overline{b-m}.d+\frac{a.\overline{c-d}.m}{2}+\frac{\overline{b-m}.\overline{c-d}.\overline{a-a'}}{2}$; in the 2d year it will be $\frac{S}{abcr^2}$ into $a''.\overline{m-n}.\overline{d-e}+a''.\overline{m-n}.e+\frac{a''.\overline{d-e}.n}{2}+\frac{\overline{m-n}.\overline{d-e}.\overline{a-a'+a''}}{2}+a'.\overline{m-n}.\overline{d-e}+\overline{b-m}.\overline{d-e}.a''+\overline{c-d}.\overline{m-n}.a''+a'.\overline{m-n}.e+\overline{b-m}.a''.e+\overline{c-d}.\overline{m-n}.\overline{a-a'+a''}+\overline{c-d}.n.a''+\frac{a'.\overline{c-d}.\overline{m-n}}{2}+\frac{\overline{b-m}.\overline{c-d}.a'}{2}$; in the 3d year it will be $\frac{S}{abcr^3}$ into $a'''.\overline{n-o}.\overline{e-f}+a''.\overline{n-o}.f+\frac{a''.\overline{e-f}.o}{2}+\overline{n-o}.\overline{e-f}.\overline{a'+a''}+\frac{\overline{n-o}.\overline{e-f}.\overline{a-a'+a''+a'''}}{2}+\overline{b-n}.\overline{e-f}.a'''+\overline{c-e}.\overline{n-o}.a'''+\overline{n-o}.f.\overline{a'+a''}+\overline{b-n}.f.a'''+\overline{c-e}.\overline{n-o}.\overline{a-a'+a''+a'''}+\overline{c-e}.o.a'''+\frac{\overline{c-e}.\overline{n-o}.\overline{a+a'}}{2}+\frac{\overline{b-n}.\overline{c-e}.a'''}{2}$, and so on.

These different fractions being added together, will be found

$=\frac{S}{b}\times\overline{\frac{b-m}{r}+\frac{m-n}{r^2}+\frac{n-o}{r^3}},\&c.-\frac{S}{2bc}\times\overline{\frac{\overline{b-m}.c}{r}+\frac{\overline{m-n}.d}{r^2}+\frac{\overline{n-o}.e}{r^3}},\&c.-\frac{S}{2bc}\times\overline{\frac{\overline{b-m}.d}{r}+\frac{\overline{m-n}.e}{r^2}+\frac{\overline{n-o}.f}{r^3}}+,\&c.+\frac{S}{2a}\times\overline{\frac{a'}{r}+\frac{a''}{r^2}+\frac{a'''}{r^3}}+,\&c.+\frac{S}{2ac}\times\overline{\frac{cd'}{r}+\frac{dd'}{r^2}+\frac{ed'''}{r^3}}+,\&c.+\frac{S}{2ab}\times\overline{\frac{ba'}{r}+\frac{ma''}{r^2}+\frac{na'''}{r^3}}+,\&c.-\frac{S}{2abc}\times\overline{\frac{bca'}{r}+\frac{mda''}{r^2}+\frac{nea'''}{r^3}}+,\&c.-\frac{S}{2abc}\times\overline{\frac{mcd'}{r}+\frac{ndd'}{r^2}+\frac{oed'''}{r^3}}+,\&c.-\frac{S}{2abc}\times\overline{\frac{mdd'}{r}+\frac{nea''}{r^2}+\frac{ofa'''}{r^3}}+,\&c.+\frac{S}{ab}\times\overline{\frac{ma'}{r}+\frac{n.\overline{d+a'}}{r^2}}+$

$\overline{\frac{o \cdot \overline{d+d'+d''}}{r^3} +}$, &c. $- \frac{S}{ab} \times \overline{\frac{bd'}{r^2} + \frac{m \cdot \overline{d'+d''}}{r^3} +}$, &c. $+ \frac{S}{2abc} \times \overline{\frac{bc \cdot d'}{r} + \frac{md \ \overline{d'+d''}}{r^3} +}$, &c. $- \frac{S}{2abc} \times \overline{\frac{mcd'}{r} + \frac{nd \cdot \overline{d'+d''}}{r^3} +}$, &c. $+ \frac{S}{2abc} \times \overline{\frac{bdd'}{r} + \frac{me \cdot \overline{d'+d''}}{r^3} +}$, &c. $- \frac{S}{2abc} \times \overline{\frac{md \cdot d'}{r} + \frac{ne \cdot \overline{d'+d''}}{r^3} +}$, &c. $+ \frac{S}{2abr} \times \overline{\frac{md}{r} + \frac{n \cdot \overline{d+d'}}{r^3}}$, &c. $- \frac{S}{2abr} \times \overline{\frac{n \cdot d}{r} + \frac{o \cdot \overline{d'+d''}}{r^3} +}$, &c. $+ \frac{S}{2abcr} \times \overline{\frac{md \cdot d}{r} + \frac{ne \cdot \overline{d'+d''}}{r^3} +}$, &c. $- \frac{S}{2abcr} \times \overline{\frac{nd \cdot d'}{r} + \frac{oe \cdot \overline{d'+d''}}{r^3}}$, &c. which may at last be reduced to S into $\frac{r-1}{r} \times \overline{V + ABC - \frac{\overline{A+B+C}}{2} + \frac{AC}{2r} - AB - \frac{n}{2c} \times \overline{AK + BK - 2ABK} + \frac{\beta}{2b} \times \overline{AF - AFC} + \frac{m}{2br} \times \overline{1 + AP} + \frac{d \cdot PT - APT}{c}}$. But, unless A and B are very nearly of the same age, and both older than C, this rule will not be sufficiently accurate. If *B be the oldest of the three lives*, the annuities A, AC, and AK, should be continued only for as many years (x) as are equal to the difference between the age of B and that of the oldest life in the table of observations. Let those annuities be respectively denoted by A′, A′C′, and A′K′; also let ϕ denote the probability that C survives B,* q the number of persons living opposite to the age of A at the end of x years, then will the value of S, after x years, be $= \frac{\phi q}{a r^x} \times \frac{\overline{r-1} \cdot \overline{V - A^x}}{r}$, and the whole value of the reversion will be $=$ S into $\frac{r-1}{r} \times \overline{V + ABC - \frac{A'+B+BC}{2} \times \frac{A'C'}{2r} - AB - \frac{n}{2c} \times \overline{A'K' + BK - 2BK} + \frac{\beta}{2b} \times \overline{AF - AFC} + \frac{m}{2br} \times \overline{1 + AP}}$

* By the Table, page 337, Phil. Trans. Vol. LXXVIII.

$+\frac{d \cdot \overline{PT-APT}}{c}+\frac{pq}{ar^x}\times\frac{\overline{r-1}\cdot\overline{V-A^x}}{r}$, ($A^x$ denoting the value of an annuity on a life x years older than A).

If *C be the oldest of the three lives*, let $\overline{a-s}$, $\overline{s-t}$, $\overline{t-u}$, &c. be substituted for their equals a', a'', a''', &c. and $\overline{c-c'}$, $\overline{c-\overline{c'+c''}}$, $\overline{c-\overline{c'+\overline{c''+c'''}}}$, &c. for their equals e, f, g. &c. then will the value of the reversion, by pursuing the same steps as in the former case, be found = S into $\frac{r-1}{r}\times$ $\overline{V-\frac{1}{2}\overline{A+B}+ABC}+\frac{BC+AC}{2r}-AB+\frac{\beta\cdot\overline{AF-AFC}}{2b}+$ $\frac{\alpha\cdot\overline{HB-HBC}}{2a}-\frac{s}{2ar}\times\overline{1+NC}-\frac{m}{2br}\times\overline{1+PC}+\frac{ms}{abr}\times$ $\overline{1+NPC}$.

But, as these series are to be continued only during C's life, it is evident that the annuities A, B, AB, &c. should also be continued only during this term; and therefore, if z be the difference between the age of C and the oldest person in the Table, A′, B′, A′B′, &c. the values of annuities respectively on the single and joint lives of A, B, A and B, &c. for z years, these several symbols should be substituted above, in lieu of A, B, AB, &c. to denote the value of the reversion during the first z years. After the extinction of the life of C, the given sum may be received upon either of three events; 1st, if A should have died before C, and B died after him in the $\overline{z+1}$, $\overline{z+2}$, &c. years; 2dly, if B should have died before C, and A died after him in those years respectively; 3dly, if both the lives of A and B should die after the first z years. Let φ denote the probability that C dies after A, and π the probability

that C dies after B,* then will the value of the reversion depending on these several contingencies (putting p for the number of persons living opposite the age of B at the end of z years, and q for the same number opposite the age of A) be $= S$ into $\frac{p \cdot \varphi \cdot \overline{r-1} \cdot \overline{V-B^{z}}}{b r^{z+1}} + \frac{q \varpi \cdot \overline{r-1} \cdot \overline{V-A^{z}}}{a r^{z+1}} + \frac{pq}{a b r^{z}} \times \overline{\frac{1+AB^{z}}{r}} - \overline{AB^{z}}$, and the whole value of the reversion will be $= S$ into $\frac{\overline{r-1} \cdot \overline{V-\frac{A'+B}{2}+ABC}}{r} + \frac{BC+AC}{2r} - AB + \frac{\beta \cdot \overline{A'F'-AFC}}{2b} + \frac{\alpha \cdot \overline{H'B'-HBC}}{2a} - \frac{s}{2ar} \times \overline{1+NC} + \frac{m}{br} \times \frac{s \cdot \overline{1+PNC}}{a} - \frac{\overline{1+PC}}{2} + \frac{p \cdot \varphi \cdot \overline{r-1} \cdot \overline{V-B^{z}}}{b r^{z+1}} + \frac{\varpi \cdot q \cdot \overline{r-1} \cdot \overline{V-A^{z}}}{a r^{z+1}} + \frac{pq}{a b r^{z+1}} \times \overline{1+AB^{z}}$.†

If the lives be all equal, the value, according to the first rule, will be $= S$ into $\frac{r-1}{r} \times \overline{V-C-\frac{1}{2}CC+CCC} + \frac{CC}{2r} - CC + \frac{x}{2c} \times \overline{KCC-KC} + \frac{d}{2cr} \times \overline{1+CT} + \frac{dd}{2ccr} \times \overline{TT-CTT}$; and, according to the 2d rule, it will be $= S$ into $\frac{r-1}{r} \times \overline{V-C+CCC} + \frac{CC}{r} - CC + \frac{x \cdot \overline{CK-CCK}}{c} - \frac{d}{cr} \times \overline{1+CT} + \frac{dd}{ccr} \times \overline{1+CTT}$. If these expressions be resolved into their respective series, the value in each case will be found $= \frac{S \cdot \overline{r-1}}{r} \times \overline{V-C-CC+CCC}$, which is known to be the true value, from self-evident principles.

But the solution of this problem may be obtained by the

* By the Table, page 229, Phil. Trans. for the year 1794.

† In this and the following problems, A^{z}, B^{z}, AB^{z}, A^{x}, B^{x}, &c. signify the value of an annuity on the single or joint lives of persons z or x years older than A and B, &c.

assistance of the 1st problem in my last paper,* supposing, instead of a *given sum*, it were required to know the value of the reversion of a given *estate*. For, since the possession of this estate is an event which must certainly take place, and the only point to be determined is the *time* in which it will probably happen, it is obvious that no event can postpone the possession, but the contingency of C's being the *second* that fails, of the three lives. If, therefore, the sum of the values of an annuity on the life of B after A, provided A should die before C, and of an annuity on the life of A after B, provided B should die before C, (both found by the problem just mentioned,) be subtracted from the whole value of the reversion after the joint lives of A and B, the remainder will be the value required. Let X and Y respectively denote the annuities found by problem 1st, (Phil. Trans. Vol. LXXXIV.) then will the general rule expressing the value of an *estate* be $= V - AB - \overline{X + Y}$, and consequently of a given *sum* $= \frac{S \cdot \overline{r-1}}{r} \times \overline{V - AB - \overline{X + Y}}$, which, when the lives are equal, may be reduced, as in the former cases, to $\frac{S \cdot \overline{r-1}}{r} \times \overline{V - C - CC + CCC}$. *Q. E. D.*

PROBLEM III.

To determine the value of an *estate*, or of a given *sum*, after the decease of A or B, should either of them be the *first* or *last* that shall fail, of the three lives, A, B, and C.

Solution.

The reversion of the *estate* in this problem, like that in the

* Phil. Trans. for the year 1794, page 235.

preceding one, cannot be prevented ultimately from taking place; and there is only the single contingency of C's being the *first* that fails, of the three lives, which can postpone the possession of it after the extinction of the joint lives. The whole value, therefore, of the reversion, after the joint lives of A and B, must in this case be lessened by the *sum* of the values of an annuity on A's life after B, provided B should survive C; and of an annuity on B's life after A, provided A should survive C, (both found by the 2d problem in my last Paper.*) Let these two values be respectively denoted by W and Z, then will the general rule expressing the value of an *estate* be $= V - AB - \overline{W + Z}$, and the value of a given *sum* $= \frac{S \cdot \overline{r-1}}{r} \times \overline{V - AB - \overline{W + Z}}$. When the lives are all *equal*, the value of the reversion, by substituting the values of W and Z, becomes $= V + CC - C - CCC$, or $\frac{S \cdot \overline{r-1}}{r} \times \overline{V + CC - C - CCC}$, according as it consists of an *estate*, or a given *sum*.

But the solution of this, like that of the preceding problem, may be obtained without having recourse to any other. In the first year, the value of the given sum will be $= S$ into $\frac{a' \cdot \overline{b-m} \cdot \overline{c-d}}{abcr} + \frac{a' \cdot \overline{b-m} \cdot d}{abcr} + \frac{a' \cdot \overline{c-d} \cdot m}{2abcr} + \frac{\overline{b-m} \cdot \overline{c-d} \cdot \overline{a-a'}}{2abcr} + \frac{a'md}{abcr} + \frac{\overline{b-m} \cdot \overline{a-a'} \cdot d}{abcr}$; in the 2d year it will be $= \frac{S}{abcr^2}$ into $\overline{m-n} \cdot \overline{d-e} \cdot a'' + \overline{m-n} \cdot e \cdot a'' + \frac{\overline{d-e} \cdot n \cdot a'}{2} + \frac{\overline{m-n} \cdot \overline{d-e} \cdot \overline{a-a' + a'}}{2} + a''ne + \overline{m-n} \cdot e \cdot \overline{a-a' + a''} + \overline{c-d} \cdot \overline{m-n} \cdot a'' + \frac{\overline{c-d} \cdot \overline{m-n} \cdot a'}{2} + \frac{\overline{b-m} \cdot \overline{c-d} \cdot a'}{2}$; in the 3d year $= \frac{S}{abcr^3}$ into

* Phil. Trans. for the year 1794, page 240.

$\overline{n-o}\,.\,\overline{e-f}\,.\,a''' + \overline{n-o}\,.\,f\,a''' + \frac{\overline{e-f}\,.\,o\,.\,a''}{2} + \frac{\overline{n-o}.\overline{e-f}.\overline{a-a'+a''+a'''}}{2}$

$+ a'''\,.\,o\,f + \overline{n-o}\,.\,f\,.\,\overline{a-a'+a''+a'''} + \overline{c-e}\,.\,\overline{n-o}\,.\,a''' + \frac{\overline{a'+a''}\,.\,\overline{c-e}\,.\,\overline{n-o}}{2} + \frac{\overline{b-n}\,.\,\overline{c-e}\,.\,a''}{2}$, and so on in the other years. If these several fractions be expanded, they will form nineteen different series, whose sum may be found $=$ S into $\frac{\overline{r-1}}{r} \times$

$\overline{V - ABC - \frac{\overline{A+B}}{2} - \frac{AC+BC}{2r} + AB + \frac{s}{2c} \times \overline{AK+BK-2ABK}}$

$- \frac{\beta\,.\,\overline{AF-AFC}}{2b} + \frac{m}{2br} \times \overline{\frac{d\,.\,\overline{1+APT}}{c} - 1 + AP}.$

If B be the oldest of the three lives, let π denote the probability that B dies *after* C,* then will the value of S, after the extinction of the life of B, be $= \frac{S\,.\,\pi\,.\,q}{a\,r^x} \times \frac{\overline{r-1}\,.\,\overline{V-A^x}}{r}$, and the whole value of the reversion will be $=$ S into $\frac{\overline{r-1}}{r} \times \overline{V - ABC -}$

$\overline{\frac{A'+B}{2} - \frac{A'C'+BC}{2r} + AB - \frac{\beta\,.\,\overline{AF-AFC}}{2b} + \frac{s}{2c} \times \overline{A'K'+BK-}}$

$\overline{2ABK} + \frac{m}{2br} \times \overline{\frac{d\,.\,\overline{1+APT}}{c} - 1 + AP} + \frac{\pi\,.\,q}{a\,r^x} \times \overline{r-1}\,.\,\overline{V-A^x}$

.... q, A′, A′C′, *&c.* denoting the same quantities as in the former part of the preceding problem.

If C be the oldest of the three lives, let the symbols be changed as in the preceding problem, and the value of the given sum will be $=$ S into $\frac{r-1}{r} \times \overline{V - ABC - \frac{1}{2}\overline{A'+B'} - \frac{\beta\,.\,\overline{A'F'-AFC}}{2b} -}$

$\overline{\frac{a\,.\,\overline{H'F'-HBC}}{2a} - \frac{ms}{abr} \times \overline{1+NPC} - \frac{AC+BC}{2r} + A'B' + \frac{m}{2br} \times}$

$\overline{1+PC} + \frac{s}{2ar} \times \overline{1+NC}$.... After the decease of C, the

* By the Table, page 229, Phil. Trans. for the year 1794.

given sum may be received, provided either of three events shall happen; 1st, if A shall have died *after* C in the first z* years, and B dies in the $\overline{z+1}\,.\,\overline{z+2}$, *&c.* year; 2dly, if B having died *after* C in the first z years, A dies in the $\overline{z+1}\,.\,\overline{z+2}$, *&c.* year; 3dly, if both A and B having survived the first z years, the survivor of them dies in any of the following years. Let π denote the probability that A dies after C, and φ the probability that B dies after C, (both found by the Table in Phil. Trans. Vol. LXXXIV. page 229) and let all the other symbols be the same as in the latter part of the preceding problem, then will the value of S, after z years, be $=$ S into $\frac{\pi\,.\,q\,.\,\overline{r-1}}{a\,r^{z+1}} \times \overline{\mathrm{V}-\mathrm{A}^2} + \frac{\varphi\,.\,p\,.\,\overline{r-1}}{b\,r^{z+1}} \times \overline{\mathrm{V}-\mathrm{B}^2} + \frac{p\,.\,q}{a\,b\,r^{z+1}} \times \overline{1-\mathrm{AB}^2} + \overline{\mathrm{A}^2+\mathrm{B}^2} - \frac{p\,q\,.}{a\,b\,r^z} \times \overline{\mathrm{A}^2+\mathrm{B}^2-\mathrm{AB}^2}$, and the whole value of the reversion will be $=$ S into $\frac{r-1}{r} \times \overline{\mathrm{V}-\mathrm{ABC}-\frac{1}{2}\mathrm{A}'+\mathrm{B}'} - \frac{\beta\,.\,\mathrm{A}'\mathrm{F}-\mathrm{AFC}}{2\,b} - \frac{\alpha\,.\,\mathrm{H}'\mathrm{F}'-\mathrm{HFC}}{2\,a} + \frac{m}{b\,r} \times \overline{\frac{1+\mathrm{PC}}{2} - \frac{s\,.\,\overline{1+\mathrm{NPC}}}{a}} - \frac{\mathrm{AC}+\mathrm{BC}}{2\,r} + \mathrm{AB} + \frac{s}{2\,a\,r} \times \overline{1+\mathrm{NC}} + \frac{\pi\,.\,q\,.\,\overline{r-1}}{a\,r^{z+1}} \times \overline{\mathrm{V}-\mathrm{A}^2} + \frac{\varphi\,p\,.\,\overline{r-1}}{b\,r^{z+1}} \times \overline{\mathrm{V}-\mathrm{B}^2} + \frac{p\,.\,q\,.\,\overline{r-1}}{a\,b\,r^{z+1}} \times \overline{\mathrm{V}-\mathrm{A}^2+\mathrm{B}^2} - \frac{p\,q}{a\,b\,r^{z+1}} \times \mathrm{AB}^2 \ldots\ldots$

If the three lives be equal, the first of these rules becomes $=$ S into $\frac{r-1}{r} \times \overline{\mathrm{V}-\mathrm{C}+\mathrm{CC}-\mathrm{CCC}} + \frac{z}{2c} \times \overline{\mathrm{CK}-\mathrm{CCK}} + \frac{dd}{2\,c\,c\,r} \times \overline{1+\mathrm{CTT}} - \frac{d}{2\,c\,r} \times \overline{1+\mathrm{CT}}$; and the second $=$ S into $\frac{r-1}{r} \times \overline{\mathrm{V}-\mathrm{C}+\mathrm{CC}-\mathrm{CCC}} - \frac{z}{c} \times \overline{\mathrm{CK}-\mathrm{CCK}} - \frac{dd}{c\,c\,r} \times \overline{1+\mathrm{CTT}} + \frac{d}{c\,r} \times \overline{1+\mathrm{CT}}$; but the three last fractions in

* z being, as usual, the number of years between the age of C and the age of the eldest person in the table.

each of those rules destroy one another, and therefore, in both of them the value of the reversion is $= \frac{S \cdot \overline{r-1}}{r} \times \sqrt{\overline{V-C+CC}} - CCC$. .... *Q. E. D.*

PROBLEM IV.

To determine the value of a given sum, payable on the death of A, should his life be the *first* or *second* that fails, and should B's life, if it fail, become extinct before the life of C.

Solution.

In this case, the payment of the given sum can only be prevented by the contingency of C's dying before A, and therefore its value is immediately found by the solution of the 2d problem in my first Paper on this subject,* being no more than "the Value of a given Sum on the Death of A, should C sur-"vive him."

The accuracy of this solution will appear from the following investigation. In the 1st year, the given sum will become payable should either of four events take place, the probabilities of which are expressed by the four fractions $\frac{a' \cdot \overline{b-m} \cdot \overline{c-d}}{2abc} + \frac{a' \cdot \overline{b-m} \cdot d}{abc} + \frac{a' m d}{abc} + \frac{a' \cdot \overline{c-d} \cdot m}{2abc} = \frac{a' c}{2ac} + \frac{a' d}{2ac}$. In the 2d year, it will become payable provided either of six events should take place, expressed by the fractions $\frac{a'' \cdot \overline{m-n} \cdot \overline{d-e}}{2abc} + \frac{a'' \cdot \overline{m-n} \cdot e}{abc} + \frac{a'' n e}{abc} + \frac{a'' \cdot \overline{d-e} \cdot n}{2abc} + \frac{\overline{b-m} \cdot \overline{d-e} \cdot a''}{2abc} + \frac{\overline{b-m} \cdot e \cdot a''}{abc} = \frac{a'' d}{2ac} + \frac{a'' e}{2ac}$. In the third year, the payment of the given sum will depend on the

* Phil. Trans. Vol. LXXVIII. page 341.

same number of events, and the probabilities of the several contingencies may be reduced to $\frac{a^n e}{2ac} + \frac{a^n f}{2ac}$. The value, therefore, of the reversion will be $= \frac{S}{2ac} \times \overline{\frac{a'c}{r} + \frac{a'.d}{r^2} + \frac{a''e}{r^3}}$, &c. $+ \frac{S}{2ac} \times \overline{\frac{a'd}{r} + \frac{a'e}{r^2} + \frac{a''f}{r^3}}$, &c. which two series are known to express the value of S, on the contingency of C's surviving A.*

Were a further proof necessary, it might be observed, that the value of the given sum, in this problem, is equal to the sum of the values of the two reversions depending on the contingency of A's being the *first* that shall fail, of the three lives; and on the contingency of C's surviving A, in case B shall be then dead. Supposing *the three lives to be equal*, these values will be $= \frac{S.\overline{r-1}}{6r} \times \overline{V - 3CC - 2CCC} + \frac{S.\overline{r-1}}{3r} \times \overline{V - CCC}$† $= \frac{S.\overline{r-1}}{2r} \times \overline{V - CC}$, or the value of the reversion depending on one life's surviving the other. ..... *Q. E. D.*

PROBLEM V.

To determine the value of a given sum, payable on the death of A, should his life be the *second* or *third* that fails, and should B's life, when it fails, become extinct before the life of C.

Solution.

The value of the given sum, in the 1st year, will be $= \frac{S}{abcr}$ into $\frac{d.\overline{b-m}.\overline{c-d}}{2} + \frac{a.\overline{b-m}.d}{2}$; in the 2d year, it will be =

* See Phil. Trans. Vol. LXXVIII. page 342.

† See Phil. Trans. Vol. LXXIX. page 49; and Vol. LXXXI. page 253.

$\frac{S}{abcr^3}$ into $\frac{a'.\overline{m-n}.\overline{d-e}}{3} + \frac{a'.\overline{m-n}.e}{2} + \overline{b-m}.\overline{d-e}.a'' +$ $\overline{b-m}.e.a'' + \frac{\overline{b-m}.\overline{c-d}.a'}{2}$; in the 3d year $= \frac{S}{abcr^3}$ into $\frac{a''.\overline{n-o}.\overline{e-f}}{3} + \frac{a''.\overline{n-o}.f}{2} + \overline{b-n}.a'''.\overline{e-f} + \overline{b-n}.a''' : f$ $+ \frac{\overline{b-n}.\overline{c-e}.a''}{2}$, and so on in the other years. These several fractions being expanded, will form seven series, whose sum will be $=$ S into $\frac{\overline{r-1}.\overline{V-A}}{2r} - \frac{\overline{r-1}.\overline{BC-ABC}}{6r} - \frac{x}{3c} \times \overline{BK - ABK} + \frac{\beta.\overline{FK-AFK}}{2b} + \frac{m.\overline{PC-APC}}{3br} - \frac{d}{6cr} \times \overline{BT - ABT} - \frac{m.\overline{PT-APT}}{b} + \frac{\beta}{2b} \times \overline{FC-AFC} - \overline{F-AF} + \frac{\overline{B-C}+\overline{AC-AB}}{2r} + \frac{x.\overline{K-AK}}{2c}$.

When B is the oldest of the three lives, let x be the number of years between the age of B and of the oldest person in the table; p the number of persons living opposite the age of C; and q the same number opposite the age of A, at the end of x years; A′, A″C, and A′K, the values of annuities on those single and joint lives for x years. The given sum may be received after the first x years, on the death of A, provided C shall have died *after* B in that time, or if C shall have survived the said term of x years. Let π denote the first probability,* and $\frac{p}{c}$ will denote the second; then will the value of this part of the reversion be $=$ S . into $\overline{\pi + \frac{p}{c}} \times \frac{q.\overline{r-1}.\overline{V-A'}}{ar^{x+1}}$, and the whole value of the reversion will be $=$ S . into $\frac{\overline{r-1}}{6r} \times \overline{2V - 3A' - ABC}$

* Found by the Table in Phil. Trans. for the year 1794, page 229.

$$-\frac{\varkappa}{3c}\times\overline{\mathrm{BK}-\mathrm{ABK}}+\frac{\beta\varkappa\,.\,\mathrm{AFK}}{6bc}+\frac{m}{3br}\times\overline{\overline{\mathrm{PC}-\mathrm{APC}}-\frac{d\,.\,1+\mathrm{APT}}{2c}}$$
$$+\frac{\beta}{6b}\times\overline{\mathrm{FC}-\mathrm{AFC}}+3\mathrm{AF}-\frac{d}{6cr}\times\overline{\mathrm{BT}-\mathrm{ABT}}-\frac{\varkappa}{2c}\times\mathrm{A'K'}$$
$$+\frac{\mathrm{A'C'}-\mathrm{AB}}{2r}+\overline{\pi+\frac{p}{c}}\times\frac{q\,.\,\overline{r-1}\,.\,\overline{\mathrm{V}-\mathrm{A}^{\varkappa}}}{ar^{\varkappa}+1}.$$

When C is the oldest of the three lives, let ϕ denote the probability that C dies after B;* let $\varkappa$ be the difference between the age of C and that of the oldest person in the table; A′, A′F′, and A′B′ the values of annuities on those single and joint lives for $\varkappa$ years; and k the number of persons living opposite a life $\varkappa$ years older than A; then will the value of the reversion in this case be = S into $\frac{r-1}{6r}\times 2\mathrm{V}-\overline{3\mathrm{A'}-\mathrm{ABC}}-\frac{\varkappa}{3c}\times\overline{\mathrm{BK}}$
$$\overline{-\mathrm{ABK}}+\frac{\beta\varkappa}{6bc}\times\mathrm{AFK}+\frac{m}{3br}\times\overline{\overline{\mathrm{PC}-\mathrm{APC}}-\frac{d\,.\,1+\mathrm{APT}}{2c}}-$$
$$\frac{d}{6cr}\times\overline{\mathrm{BT}-\mathrm{ABT}}+\frac{\beta}{6b}\times\overline{\mathrm{FC}-\mathrm{AFC}}+3\mathrm{A'F'}-\frac{\varkappa}{2c}\times$$
$$\mathrm{AK}+\frac{\mathrm{AC}-\mathrm{A'B'}}{2r}+\frac{\phi k\,.\,\overline{r-1}\,.\,\overline{\mathrm{V}-\mathrm{A}^{\varkappa}}}{ar^{\varkappa}+1}.$$

If A be the oldest of the three lives, let b', b'', b''', &c. be substituted for $\overline{b-m}$, $\overline{m-n}$, $\overline{n-o}$, &c. and $\overline{a-s}$, $\overline{s-t}$, &c. for a', a'', &c. then will the value of the given sum for the 1st year be = $\frac{\mathrm{S}}{abcr}\times\frac{\overline{a-s}\,.\,\overline{c-d}\,.\,b'}{3}+\frac{\overline{a-s}\,.\,db'}{2}$; for the 2d year =
$$\frac{\mathrm{S}}{abcr^{2}}\times\overline{\frac{\overline{s-t}\,.\,\overline{d-e}\,.\,b''}{3}+\frac{\overline{s-t}\,.\,eb''}{2}+\overline{s-t}\,.\,\overline{d-e}\,.\,b'+\overline{s-t}\,.\,eb'}$$
$$\overline{+\frac{\overline{c-d}\,.\,\overline{s-t}\,.\,b'}{2}}, \text{ and so on for the other years. Hence the whole}$$
value may be found = S into $\frac{\overline{r-1}}{6r}\times\overline{2\mathrm{V}-3\mathrm{A}-\mathrm{ABC}}-\frac{\varkappa}{3c}$

* By the Table in Phil. Trans. for the year 1794, page 229.

$\times \overline{AK - ABK} - \frac{ss \cdot HBK}{3ac} + \frac{s}{6a} \times \overline{HC - HBC} - \frac{d}{6cr} \times \overline{\overline{AT - \overline{ABT}}} + \frac{s \cdot 1 + NBT}{a} - \frac{s}{2ar} \times \overline{\frac{NC - NBC}{3} - 1 + NB} + \frac{AC - AB}{2r}$.

When the lives are equal, the value, by the two first rules, will be $= \frac{S \cdot r-1}{6r} \times \overline{2V - 3\overline{C - CCC}} + S \text{ into} - \frac{s}{6c} + \overline{CK - CCK} + \frac{ss}{6cc} CKK + \frac{d}{6cr} \times \overline{CT - CCT} - \frac{dd}{6ccr} \times \overline{1 + CTT}$; and by the third rule it will be $= S$ into $\frac{r-1}{6r} \times \overline{2V - 3\overline{C - CCC}} - \frac{ss}{6cc} \times CKK + \frac{d}{6cr} \times CT + \frac{d}{3cr} \times CCT + \frac{d}{2cr} - \frac{dd}{6ccr} \times \overline{1 + CTT}$. In both cases, all the fractions after the first destroy each other, so that the general rule is $= \frac{S \cdot \overline{r-1}}{6r} \times \overline{2V - 3\overline{C - CCC}}$, which may be proved, from other principles, to be the true value.

As the reversion, in this problem, consists of two parts; 1st, of the contingency of receiving the given sum on the death of A, provided B should be then dead and C living; and 2dly, of the contingency of receiving it on the death of A, provided B should be the first, C the second, and A the third, that fails, it follows, that the present solution may be obtained from those of the problem in my second Paper,* and of the 6th problem† in my last Paper on this subject. But the computations derived from the addition of those two problems would be too tedious and complicated, and therefore the preceding rules are preferable. In the particular case of the *equality* of the lives, the general rule becomes the same as above, (or $= S \times \frac{r-1}{6r} \times$

* Phil. Trans. Vol. LXXIX. page 41. † Phil. Trans. for the year 1794, page 253.

$\overline{2V - 3C - CCC}$) and consequently affords an additional proof of the truth of the investigation of the three problems.

PROBLEM VI.

To determine the value of a given sum, payable on the death of A, should his life be the *first* or *last* that shall fail, of the three lives; and should B's life, if it fail, become extinct before the life of C.

Solution.

The value of the given sum, in the 1st year, is $\frac{S}{abcr}$ into $\frac{a' \cdot \overline{b-m} \cdot \overline{c-d}}{2} + a'md + \frac{a' \cdot \overline{b-m} \cdot d}{2} + \frac{a' \cdot \overline{c-d} \cdot m}{2}$; in the 2d year, its value is $= \frac{S}{abcr^2}$ into $\frac{a'' \cdot \overline{m-n} \cdot \overline{d-e}}{2} + a''ne + \frac{a'' \cdot \overline{m-n} \cdot e}{2} + \frac{a'' \cdot \overline{d-e} \cdot n}{2} + \frac{\overline{b-m} \cdot \overline{d-e} \cdot a''}{2} + \frac{\overline{b-m} \cdot \overline{c-d} \cdot a''}{2}$; in the 3d year, $\frac{S}{abcr^3}$ into $\frac{a''' \cdot \overline{n-o} \cdot \overline{e-f}}{2} + a'''of + \frac{a''' \cdot \overline{n-o} \cdot f}{2} + \frac{a''' \cdot \overline{e-f} \cdot o}{2} + \frac{\overline{b-n} \cdot \overline{e-f} \cdot a'''}{2} + \frac{\overline{b-n} \cdot \overline{c-e} \cdot a'''}{2}$, and so on in the other years. The sum of these several fractions, after reducing them into their proper series, may be found $= S$ into $\frac{r-1}{2r} \times \overline{V - A - ABC} + \frac{AC}{2} - \frac{AB}{2r} + \frac{md}{2bcr} \times \overline{1 + APT} - \frac{d}{2cr} \times \overline{1 + AT + BT - ABT} - \frac{\beta_n}{2bc} \times AFK + \frac{\beta}{2b} \times \overline{AF + FC - AFC}$.

When B is the oldest of the three lives, let x be the difference between his age and that of the oldest person in the table; and, as the given sum may be received after the necessary extinction of his life, either on the event of C's having died *after* him in x

years, and A's dying in the $\overline{x+1}$, $\overline{x+2}$, *&c.* years, or on the event of C's having lived x years, and A's dying in the $\overline{x+1}$, $\overline{x+2}$, *&c.* years, it is obvious that the preceding rule in this case will not express the whole value of the reversion. Retaining the same symbols as in the first case of Prob. V. the value depending on the former of those two events will be $= S \times \frac{s \cdot q \cdot \overline{r-1} \cdot \overline{V-A^x}}{a r^x+1}$, and on the latter of them it will be $=$

$\frac{AC^x}{2r^x+1} - \frac{d}{2cr^x+1} \times AT^x + \frac{pq \cdot \overline{r-1} \cdot \overline{V-A^x}}{acr^x+1} - \frac{pq \cdot \overline{r-1} \cdot \overline{V-AC^x}}{2acr^x+1}$;

therefore, the whole value of the reversion will be $=$ S into

$\frac{r-1}{2r} \times \overline{V-A'-ABC} + \frac{AC}{2} - \frac{AB}{2r} + \frac{md}{2bcr} \times \overline{1+APT} -$

$\frac{d}{2cr} \times \overline{1+AT+\overline{BT-ABT}} - \frac{\beta x}{2bc} \times AFK + \frac{\beta}{2b} \times \overline{AF}$

$+ \overline{FC-AFC} - \frac{pq \cdot \overline{r-1} \cdot \overline{V-AC^x}}{2acr^x+1} + \overline{\pi + \frac{p}{c}} \times \frac{q \cdot \overline{r-1} \cdot \overline{V-A^x}}{ar^x+1}$.

When C is the oldest of the three lives, let ϕ denote the probability that C dies after B;* and let z be the difference between the age of C and that of the oldest person in the table; A', A'B', and A'F', the values of annuities on those single and joint lives for z years; and q the number of persons living at the age of A after z years; then will the value in this case be $=$ S into

$\frac{r-1}{2r} \times \overline{V-A'-ABC} + \frac{AC}{2} - \frac{A'B'}{2r} + \frac{md}{2bcr} \times \overline{1+APT} - \frac{d}{2cr}$

$\times \overline{1+AT+\overline{BT-ABT}} - \frac{\beta x}{2bc} \times AFK + \frac{\beta}{2b} \times \overline{A'F'+FC}$

$- \overline{AFC} + \frac{\phi q \cdot \overline{r-1} \cdot \overline{V-A^x}}{ar^x+1}$.

When A is the oldest of the three lives, let the symbols be changed in like manner as in the corresponding case in the

* Phil. Trans. for the year 1794, page 229.

preceding problem; then will the value for the 1st year be $= \frac{S}{abcr}$ into $\frac{\overline{a-s}\cdot\overline{c-d}\cdot b'}{2} + \overline{a-s}\cdot\overline{b-b'}\cdot d + \frac{\overline{a-s}\cdot d\,b'}{2} + \frac{\overline{a-s}\cdot\overline{c-d}\cdot\overline{b-b'}}{2}$; for the 2d year $= \frac{S}{abcr^2}$ into $\frac{\overline{s-t}\cdot\overline{d-e}\cdot b''}{2} + \overline{s-t}\cdot\overline{b-b'+b''}\cdot e + \frac{\overline{s-t}\cdot e\,b''}{2} + \frac{\overline{s-t}\cdot\overline{d-e}\cdot\overline{b-b'+b''}}{2} + \frac{\overline{s-t}\cdot\overline{d-e}\cdot b'}{2} + \frac{\overline{s-t}\cdot\overline{c-d}\cdot b'}{2}$, and so on for the 3d, 4th, and remaining years. These several fractions may be expanded into twelve different series, whose sum may be found $=$ S into $\frac{r-1}{2r} \times \overline{V-A-ABC} + \frac{AC}{2} - \frac{AB}{2r} - \frac{s\cdot AK}{2c} + \frac{a\cdot HBC}{2a} - \frac{d}{2cr} \times \overline{AT-ABT} + \frac{s}{2ar} \times \overline{1+NC+NB-NBC} - \frac{ds}{2acr} \times \overline{1+NBT}$.

If the lives be equal, the value, by the first two rules, will be $=$ S into $\frac{r-1}{2r}\cdot\overline{V-C+CC-CCC} + \frac{dd}{2ccr} \times \overline{1+CTT} - \frac{d}{2cr} \times \overline{1+2CT-CCT} - \frac{ss\cdot CKK}{2cc} + \frac{s}{2c} \times \overline{2CK-CCK}$; and by the third rule it will be $=$ S into $\frac{\overline{r-1}\cdot\overline{V-C+CC-CCC}}{2r} + \frac{s}{2c}\cdot\overline{CCK-CK} + \frac{d}{2cr} \times \overline{1+CT} - \frac{dd}{2ccr} \times \overline{1+CTT}$. In both cases, all the fractions after the first destroy each other; so that the general rule becomes $= \frac{S\cdot\overline{r-1}}{2r} \times \overline{V-C+CC-CCC}$, which may be proved to be the true value, from other principles.

The solution of this problem might have been derived from that of the first problem* in my third Paper, and from that of the 6th problem† in my last Paper on this subject, "by finding "the value on the death of A, should his life be the *first* that

* Phil. Trans. Vol. LXXXI. page 248.

† Phil. Trans. for the year 1794, page 253.

"failed; and also the value of the same sum, should B be the "first, C the second, and A the third, that failed:" but the foregoing rules, in being more simple, are preferable.

In the particular case of the *equality of the three lives*, the value, by these problems, will be = S into $\frac{r-1}{3r} \times \overline{V - CCC} + \frac{r-1}{6r} \times \overline{V - 3C + 3CC - CCC} = \frac{S \cdot \overline{r-1}}{2r} \times \overline{V - C + CC - CCC}$, as before. *Q. E. D.*

PROBLEM VII.

To determine the value of a given sum, payable on the death of A, B, and C, provided C shall die after one life in particular, (A).

Solution.

In the 1st year, the value of the given sum will be = $\frac{S}{2abcr} \times \overline{b-m} \cdot \overline{c-d} \cdot a'$; in the 2d year, it will be = $\frac{S}{abcr^2}$ into $\frac{\overline{m-n} \cdot \overline{d-e} \cdot a'}{2} + \overline{b-m} \cdot \overline{d-e} \cdot a' + \frac{\overline{c-d} \cdot \overline{m-n} \cdot a'}{2} + \overline{m-n} \cdot \overline{d-e} \cdot a' + \frac{\overline{b-m} \cdot \overline{d-e} \cdot a'}{2}$; in the 3d year = $\frac{S}{abcr^3}$ into $\frac{\overline{n-o} \cdot \overline{e-f} \cdot a''}{2} + \overline{b-n} \cdot \overline{e-f} \cdot \overline{a' + a''} + \frac{\overline{c-e} \cdot \overline{n-o} \cdot \overline{a' + a''}}{2} + \overline{n-o} \cdot \overline{e-f} \cdot \overline{a' + a''} + \frac{\overline{b-n} \cdot \overline{e-f} \cdot a''}{2}$, and so on in the other years; whence the whole value may at last be found = S into $\frac{\overline{r-1}}{2r} \times \overline{BC - B - ABC} + \frac{BC}{2} - \frac{AB}{2r} + \frac{m}{2br} \times \overline{1 + AP - \frac{d \cdot \overline{1 + APT}}{c}} - \frac{n}{2c} \times \overline{BK - ABK} + R$, (R denoting the value of S, by the 3d problem

in my first Paper, on the contingency of C's dying *after* A).* This general rule gives the true value of the reversion, *when B is the oldest of the three lives.* But, *when C is the oldest of the three lives,* the general rule will be $= S$ into $\frac{r-1}{2r} \times \overline{BC - B'} \overline{- ABC} + \frac{BC}{2} - \frac{A'B'}{2r} + \frac{m}{2br} \times \overline{1 + A'P' - \frac{d \cdot \overline{1+AP}T}{c}} - \frac{x}{2c} \times \overline{BK - ABK} + R + \frac{\mu p \cdot \overline{r-1} \cdot \overline{V-B'}}{br^{x+1}}$. $-x$ denoting the difference between the ages of C and of the oldest person in the table; p the number of persons living at the age of B after x years; B', A'B', and A'P', the values of annuities on those single and joint lives for x years; and μ the probability that C dies *after* A.†

When A is the oldest of the three lives, let the symbols be changed as in the solution of the preceding problem, and the whole value of the given sum will be $= \frac{S}{2b} \times \overline{\frac{b'}{r} + \frac{b''}{r^2} + \frac{b'''}{r^3}}$, &c. $+ \frac{S}{2bc} \times \overline{\frac{cb'}{r} + \frac{db''}{r^2} + \frac{eb'''}{r^3}}$, &c. $+ \frac{S}{2bcr} \times \overline{\frac{d'b'}{r} + \frac{e \cdot \overline{b'+b''}}{r^2}}$ +, &c. $- \frac{S}{bc} \times \overline{\frac{db'}{r} + \frac{eb''}{r^2} + \frac{f \cdot b'''}{r^3}}$ +, &c. $- \frac{S}{bcr} \times \overline{\frac{eb'}{r} + \frac{f \cdot \overline{b'+b''}}{r^2}}$ +, &c. $- \frac{S}{2ab} \times \overline{\frac{ab'}{r} + \frac{s \cdot b''}{r^2} + \frac{tb'''}{r^3}}$ +, &c. $- \frac{S}{2abc} \times \overline{\frac{scb'}{r} + \frac{dtb''}{r^2} + \frac{eub'''}{r^3}}$ +, &c. $- \frac{S}{2abcr} \times \overline{\frac{dtb'}{r} + \frac{eu \cdot \overline{b'+b''}}{r^2}}$ +, &c. $+ \frac{S}{2abc} \times \overline{\frac{ab'd}{r} + \frac{sb'e}{r^2} + \frac{tb''f}{r^3}}$, &c. $+ \frac{S}{2abcr} \times \overline{\frac{secb'}{r} + \frac{tf \cdot \overline{b'+b''}}{r^2}}$ +, &c. $+ \frac{S}{2abc} \times \overline{\frac{zdb'}{r} + \frac{teb''}{r^2} + \frac{ufb'''}{r^3}}$ +, &c. $+ \frac{S}{2abcr} \times \overline{\frac{teb'}{r} + \frac{uf \cdot \overline{b'+b''}}{r^2}}$, &c. $- \frac{S}{2abcr} \times \overline{\frac{dsb'}{r} + \frac{te \cdot \overline{b+b'}}{r^2}}$ +, &c. ....

* Phil. Trans. Vol. LXXVIII. page 347.

† Phil. Trans. for the year 1794, table in page 239.

After the extinction of A's life, the given sum may be received on either of three events; 1st. on the death of B in the $\overline{z+1}$, $\overline{z+2}$, &c. years, (z denoting the difference between the ages of A and of the oldest person in the table,) C having died *after* A in the first z years; 2dly, on the death of C in the $\overline{z+1}$, $\overline{z+2}$, &c. years, B having died in the first z years; 3dly, on the extinction of both the lives of B and C after the first z years. Let ϕ denote the probability that C dies after A in z years;* p the number of persons living opposite the age of B; and k the same number opposite the age of C at the end of z years; and the value on the two first of these contingencies will be $= \frac{S \cdot \phi p \cdot \overline{r-1} \cdot \overline{V-B^r}}{br^{z+1}} + \frac{S \cdot \overline{b-p} \cdot k \cdot \overline{r-1} \cdot \overline{V-C^r}}{bcr^{z+1}}$. Again, let *greek* letters be substituted for the corresponding *italic* letters in the first part, and the value on the third of those contingencies will be $= \frac{S}{br^z} \times \overline{\frac{\beta}{r} + \frac{\beta'}{r^2} + \frac{\beta''}{r^3}}$, &c. $- \frac{S}{bcr^z} \times \overline{\frac{\beta\beta}{r} + \frac{\cdot\beta\beta'}{r^2} + \frac{\zeta\beta''}{r^3}}$, &c. $+ \frac{S}{bcr^{z+1}} \times \overline{\frac{\beta\beta}{r} + \frac{\cdot 1 \cdot \overline{\beta+\beta'}}{r^2}}$, &c. $- \frac{S}{bcr^{z+1}} \times \overline{\frac{\cdot\beta\beta}{r} + \frac{\zeta \cdot \overline{\beta+\beta'}}{r^2}}$, &c. The 1st of these series being added to the first series in the former part of the solution, their sum will be $=$ S into $\frac{\overline{r-1} \cdot \overline{V-B}}{2r} + \frac{p \cdot \overline{r-1} \cdot \overline{V-B^r}}{abr^{z+1}}$; the 2d and 4th being added to the 4th and 5th, their sum will be $= -$ S $\times \overline{C-BC}$; and the 3d series being added to the 3d series in that part of the solution, their sum will be $= \frac{S \cdot \overline{C-BC}}{r}$; so that these six series last mentioned are $= - \frac{S \cdot \overline{r-1} \cdot \overline{C-BC}}{r}$.† The whole value of

* Phil. Trans. for the year 1794, table in page 229.

† The solution of the latter part of the case in the 6th problem, in which B is the

the reversion in this case may therefore be found $=$ S . into $\frac{r-1}{2r} \times \overline{V - B - 2C + 2BC + AC - ABC} + \frac{B'C - AB}{2r} + \frac{a}{2a} \times \overline{HB + HC - HBC} - \frac{x}{2c} \times \overline{B'K' + AK - ABK} + \overline{\varphi + \frac{1}{2}} \times \frac{b \cdot r-1 \cdot \overline{V-B}}{br+1} + \frac{\overline{b-b} \cdot k \cdot \overline{r-1} \cdot \overline{V-C}}{bcr+1}$; B′C′, and B′K′, denoting the values of annuities on those joint lives for x years.

When the three lives are of equal age, the value by the first two rules will be $=$ S into $\frac{r-1}{2r} \times \overline{\overline{V - 3C + 3CC - CCC} + \frac{d}{2cr} \times 1 + CT - \frac{dd}{2ccr} \times 1 + CTT - \frac{x}{2c} \times \overline{CK - CCK}}$, in which the last three fractions destroy each other; and by the last rule the value will be $= \frac{S \cdot \overline{r-1}}{2r} \times \overline{V - 3C + 3CC - CCC}$; that is, in both cases, "half the reversion after the extinction of "the three lives," which from self-evident principles is known to be the true value.

The solution of this problem may also be derived from those of the 3d problem in my first Paper,* and of the 1st problem in my last Paper,† "by deducting the value of an *estate* after the "death of C, provided that should happen after the death of A, "from the value of an annuity on the life of B after C, pro-"vided C should die before A." Thus, in the case of *equal* lives, the value by the first of these problems being $\frac{V - 2C + CC}{2}$, and the value by the second being $= \frac{C - CC}{2} - \frac{CC - CCC}{2}$, their difference, or $\frac{V - 3C + 3CC - CCC}{2}$, is the number of years pur-

eldest of the three lives, has been investigated much in the same manner with the present case; but the operation was omitted merely for the sake of conciseness.

* Phil. Trans. Vol. LXXVIII. page 347.

† Phil. Trans. for the year 1794, page 235.

chase required, and consequently the value of a given sum is $= S \times \frac{r-1}{2r} \times \sqrt{-3C+3CC-CCC}$, as before. But the rules derived from the foregoing solution are in general more simple than those derived from the two problems just mentioned, and are therefore to be preferred to them.

The foregoing problems, together with those which have been investigated in my former papers, comprehend, as far as I can perceive, all the different cases of survivorship between three lives. The great number of contingencies on which these reversions depend, must necessarily render the solutions intricate, and consequently the general rules complicated and laborious. It would not, however, be a difficult task to abridge these rules very considerably, without destroying their accuracy in any great degree; but this would be foreign to my purpose in these papers, which has uniformly been confined to the investigation of the *correct* values of the different reversions. Nor do I think that such an abridgement is necessary, as the operations of even the longest of the present rules, may be completed in very nearly as short a time as the inaccurate approximations which have hitherto been employed for the same purpose.

It may not be improper to observe, that the solutions in these papers are not only the first which have ever been deduced, in the case of two and three lives, from just principles and the real probabilities of life; but that, as to many of the problems, not even an attempt has ever been made to *approximate* to the value of the reversion.

Being now possessed of correct solutions of all the cases in which two and three lives are involved in the survivorship, we are possessed of all that is really useful, and therefore I feel the

greater satisfaction in closing my inquiries on this subject. For, in regard to contingencies depending on four or more lives, the cases are not only much too numerous and intricate to admit of a solution, but they occur so seldom in practice, as to render the entire investigation of them, were it even possible, a matter of little or no importance.

In my last Paper, printed in the Phil. Trans. for the year 1794, page 257, last line but one,

for $-\frac{BC-B'C'}{2r}$, read $-\frac{pk.\overline{r-1}.\overline{V-BC^{m}}}{2bcr^{m+1}}$; whence the general rule in the following page becomes $= S$ into $\frac{r-1}{6r} \times \overline{V-3C-ABC} + \frac{BC}{2} - \frac{AC}{2r} - \frac{\beta}{3b} \times \overline{\frac{AF-AFC}{3} + \frac{a.HFC}{3a} + \frac{a}{2a} \times \overline{\frac{2.HB-HBC}{3} + HC} + \frac{s}{6ar} \times \overline{NB-NBC} + \overline{\frac{m.1+NPC}{b} - \frac{m}{2br} \times \overline{1+PC} + \frac{2.AP-APC}{3}} + w + \frac{p}{b} \times \frac{k.\overline{r-1}.\overline{V-C^{m}}}{cr^{m+1}} - \frac{pk.\overline{r-1}.\overline{V-BC^{m}}}{2bcr^{m+1}}}$.

III. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, *for the Year* 1798. *By* Thomas Barker, *Esq.*

Read December 12, 1799.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.
Jan.	Morn.	30,01	28,47	29,44	46½	33	40½	49	28	36½	1,028
	Aftern.				47½	34	41	49	30	40½	
Feb.	Morn.	30,19	28,70	58	49½	35½	41½	50	23½	34½	1,542
	Aftern.				50	37	43	54	32½	43	
Mar.	Morn.	29,84	28,71	44	51½	40	43	48½	28	38	0,532
	Aftern.				53	41½	44	58	36½	44½	
Apr.	Morn.	29,88	28,66	48	58	39	51	52½	29	46	1,321
	Aftern.				61	41	51½	68	44½	57	
May	Morn.	30,00	28,75	56	61½	51	55	62½	44	52	1,892
	Aftern.				64	53½	58½	71	52½	61½	
June	Morn.	29,98	29,11	61	68	56	62	69	51	59½	0,950
	Aftern.				70	57½	64	84	60	72	
July	Morn.	29,73	28,91	33	66	57	61	67	54	59	2,942
	Aftern.				69	59½	63	79	60½	70	
Aug.	Morn.	29,90	29,25	62	66	60	63	64½	51	58½	1,942
	Aftern.				70	61½	65	80	62½	70	
Sep.	Morn.	29,77	28,48	32	66	52	58½	62½	44	53	2,814
	Aftern.				68½	51	60½	76	48	63½	
Oct.	Morn.	29,87	28,70	39	57½	44	52	55	32	47	3,036
	Aftern.				60	44	53	63	43½	55	
Nov.	Morn.	29,75	28,21	09	52	35½	42½	53	27	38	2,546
	Aftern.				53	36	43½	57	33	43	
Dec.	Morn.	30,18	28,80	43	42	21½	36	42½	5½	31½	1,396
	Aftern.				43	23½	36½	45	13	35	

The year began open and mild, and drier than it had been at the end of last year. There was near a week's frost about January the 10th; again about February the 6th; and the sharpest this winter was about the 18th, but none of them lasted a week. It was in general an open, mild, and pleasant winter, and drying, except one great rain and flood, February 23. The NE winds in the middle and latter end of March delayed the grass, but the season was in general calm. It was a fine April, chiefly dry, though with some fine rains at times, and not without some storms. A fruitful season, though in some measure hindered by frequent frosty mornings, for the first three weeks of April: this brought the grain on finely; and the low meadows, which were so thoroughly soaked by the wet last winter, had great crops of grass; but the late laid uplands were thin.

I think it was upon the whole one of the finest summers I have known; a great deal of calm, sunny, and fine weather, and moderately hot. The wet of the winter prevented the ground from burning till the middle of the summer: then a wet middle of July set the grass growing again; but, as rain was wanted, it was not complained of, though in hay-time; for it rather hindered than damaged the hay, which was for the most part pretty well got, though rather troublesome, and the wet made plenty of turnips, and fine eddishes. The harvest was in general well got, with good crops of white corn; but the weather had been full dry for the beans and peas, which were thin, though pretty well corned; and the harvest was early, for it was almost finished in August. The fine harvest made the ground begin to burn again in September; but that was soon stopped, by a great and windy rain before the end of

the month. The autumn was fine, with few frosty mornings; yet one or two severe nights in October, before the green-house plants were housed, killed many of them, especially the Geraniums: but it was afterwards warm and wet again, and continued pretty fine, for the season, till the middle of December, when, after some misty weather, followed by very severe frost in the last week, it was exceedingly cold, and the thermometer was one day down at $5\frac{1}{2}$, which I never saw it at but once before; and the frost continued, though not so severe, till near the middle of January, 1799.

IV. *On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations.* By William Herschel, *LL. D. F. R. S.*

Read November 21, 1799.

It will not be difficult to shew that the power of penetrating into space by telescopes is very different from magnifying power, and that, in the construction of instruments, these two powers ought to be considered separately.

In order to conduct our present inquiry properly, it will be necessary to examine the nature of luminous bodies, and to enter into the method of vision at a distance. Therefore, to prevent the inaccuracy that would unavoidably arise from the use of terms in their common acceptation, I shall have recourse to algebraic symbols, and to such definitions as may be necessary to fix a precise meaning to some expressions which are often used in conversation, without much regard to accuracy.

By luminous bodies I mean, in the following pages, to denote such as throw out light, whatever may be the cause of it: even those that are opaque, when they are in a situation to reflect light, should be understood to be included; as objects of vision they must throw out light, and become intitled to be called luminous. However, those that shine by their own light may

be called self-luminous, when there is an occasion to distinguish them.

The question will arise, whether luminous bodies scatter light in all directions equally; but, till we are more intimately acquainted with the powers which emit and reflect light, we shall probably remain ignorant on this head. I should remark, that what I mean to say, relates only to the physical points into which we may conceive the surfaces of luminous bodies to be divided; for, when we take any given luminous body in its whole construction, such as the sun or the moon, the question will assume another form, as will appear hereafter.

That light, flame, and luminous gases are penetrable to the rays of light, we know from experience;* it follows therefore, that every part of the sun's disk cannot appear equally luminous to an observer in a given situation, on account of the unequal depth of its luminous atmosphere in different places.† This regards only bodies that are self-luminous. But the greatest inequalities in the brightness of luminous bodies in general, will undoubtedly be owing to their natural texture;

* In order to put this to a proof, I placed four candles behind a screen, at $\frac{1}{2}$ of an inch distance from each other, so that their flames might range exactly in a line. The first of the candles was placed at the same distance from the screen, and just opposite a narrow slit, $\frac{3}{4}$ of an inch long, and $\frac{1}{4}$ broad. On the other side of the screen I fixed up a book, at such a distance from the slit that, when the first of the candles was lighted, the letters might not be sufficiently illuminated to become legible. Then, lighting successively the second, third, and fourth candles, I found the letters gradually more illuminated, so that at last I could read them with great facility; and, by the arrangement of the screen and candles, the light of the second, third, and fourth, could not reach the book, without penetrating the flames of those that were placed before them.

† See the Paper on the Nature and Construction of the Sun. Phil. Trans. for 1795, page 46.

which may be extremely various, with regard to their power of throwing out light more or less copiously.

Brightness, I ascribe to bodies that throw out light; and those that throw out most are the brightest.

It will now be necessary to establish certain expressions for brightness in different circumstances.

In the first place, let us suppose a luminous surface throwing out light, and let the whole quantity of light thrown out by it be called L.

Now, since every part of this surface throws out light, let us suppose it divided into a number of luminous physical points, denoted by N.

If the copiousness of the emission of light from every physical point of the luminous surface were equal, it might in general be denoted by c; but, as that is most probably never the case, I make C stand for the mean copiousness of light thrown out from all the physical points of a luminous object. This may be found in the following manner. Let c express the copiousness of emitting light, of any number of physical points that agree in this respect; and let the number of these points be n. Let the copiousness of emission of another number of points be c', and their number n'. And if, in the same manner, other degrees of copiousness be called c'', c''', &c. and their numbers be denoted by n'', n''', &c. then will the sum of every set of points, multiplied by their respective copiousness of emitting light, give us the quantity of light thrown out by the whole luminous body. That is, $L = cn + c'n' + c''n''$, &c.; and the mean copiousness of emitting light, of each physical point, will be expressed by $\frac{cn+c'n'+c''n'', \&c.}{N} = C$.

It is evident that the mean power, or copiousness of throwing out light, of every physical point in the luminous surface, multiplied by the number of points, must give us the whole power of throwing out light, of the luminous body. That is $CN = L$.

I ought now to answer an objection that may be made to this theory. Light, as has been stated, is transparent; and, since the light of a point behind the surface of a flame will pass through the surface, ought we not to take in its depth, as well as its superficial dimensions? In answer to this, I recur to what has been said with regard to the different powers of throwing out light, of the points of a luminous surface. For, as light must be finally emitted through the surface, it is but referring all light arising from the emission of points behind the surface, to the surface itself, and the account of emitted light will be equally true. And this will also explain why it has been stated as probable, that different parts of the same luminous surface may throw out different quantities of light.

Since, therefore, the quantity of light thrown out by any luminous body is truly represented by CN, and that an object is bright in consequence of light thrown out, we may say that brightness is truly defined by CN. If however, there should at any time be occasion for distinction, the brightness arising from the great value of C, may be called the intrinsic brightness; and that arising from the great value of N, the aggregate brightness; but the absolute brightness, in all cases, will still be defined by CN.

Hitherto we have only considered luminous objects, and their condition with regard to throwing out light. We proceed now to find an expression for their appearance at any assigned distance; and here it will be proper to leave out of the account,

every part of CN which is not applied for the purpose of vision. L representing the whole quantity of light thrown out by CN, we shall denote that part of it which is used in vision, either by the eye or by the telescope, l. This will render the conclusions that may be drawn hereafter more unexceptionable; for, the quantity of light l being scattered over a small space in proportion to L, it may reasonably be looked upon as more uniform in its texture; and no scruples about its inequality will take place. The equation of light, in this present sense, therefore, is $CN = l$.

Now, since we know that the density of light decreases in the ratio of the squares of the distances of the luminous objects, the expression for its quantity at the distance of the observer D, will be $\frac{l}{D^2}$.

In natural vision, the quantity l undergoes a considerable change, by the opening and contracting of the pupil of the eye. If we call the aperture of the iris a, we find that in different persons it differs considerably. Its changes are not easily to be ascertained; but we shall not be much out in stating its variations to be chiefly between 1 and 2 tenths of an inch. Perhaps this may be supposed under-rated; for the powers of vision, in a room completely darkened, will exert themselves in a very extraordinary manner. In some experiments on light, made at Bath, in the year 1780, I have often remarked, that after staying some time in a room fitted up for these experiments, where on entering I could not perceive any one object, I was no longer at a loss, in half an hour's time, to find every thing I wanted. It is however probable that the opening of the iris is not the only cause of seeing better after remaining long in the dark; but

that the tranquillity of the retina, which is not disturbed by foreign objects of vision, may render it fit to receive impressions such as otherwise would have been too faint to be perceived. This seems to be supported by telescopic vision; for it has often happened to me, in a fine winter's evening, when, at midnight, and in the absence of the moon, I have taken sweeps of the heavens, of four, five, or six hours duration, that the sensibility of the eye, in consequence of the exclusion of light from surrounding objects, by means of a black hood which I wear upon these occasions, has been very great; and it is evident, that the opening of the iris would have been of no service in these cases, on account of the diameter of the optic pencil, which, in the 20 feet telescope, at the time of sweeping, was no more than ,12 inch. The effect of this increased sensibility was such, that if a star of the 3d magnitude came towards the field of view, I found it necessary to withdraw the eye before its entrance, in order not to injure the delicacy of vision acquired by long continuance in the dark. The transit of large stars, unless where none of the 6th or 7th magnitude could be had, have generally been declined in my sweeps, even with the 20 feet telescope. And I remember, that after a considerable sweep with the 40 feet instrument, the appearance of Sirius announced itself, at a great distance, like the dawn of the morning, and came on by degrees, increasing in brightness, till this brilliant star at last entered the field of view of the telescope, with all the splendour of the rising sun, and forced me to take the eye from that beautiful sight. Such striking effects are a sufficient proof of the great sensibility of the eye, acquired by keeping it from the light.

On taking notice, in the beginning of sweeps, of the time that passed, I found that the eye, coming from the light, required

near 20′, before it could be sufficiently reposed to admit a view of very delicate objects in the telescope; and that the observation of a transit of a star of the 2d or 3d magnitude, would disorder the eye again, so as to require nearly the same time for the re-establishment of its tranquillity.

The difficulty of ascertaining the greatest opening of the eye, arises from the impossibility of measuring it at the time of its extreme dilatation, which can only happen when every thing is completely dark; but, if the variation of a is not easily to be ascertained, we have, on the other hand, no difficulty to determine the quantity of light admitted through a telescope, which must depend upon the diameter of the object-glass, or mirror; for, its aperture A may at all times be had by measurement.

It follows, therefore, that the expression $\frac{a^2 l}{D^2}$ will always be accurate for the quantity of light admitted by the eye; and that $\frac{A^2 l}{D^2}$ will be sufficiently so for the telescope. For it must be remembered, that the aperture of the eye is also concerned in viewing with telescopes; and that, consequently, whenever the pencil of light transmitted to the eye by optical instruments exceeds the aperture of the pupil, much light must be lost. In that case, the expression $A^2 l$ will fail; and therefore, in general, if m be the magnifying power, $\frac{A}{m}$ ought not to exceed a.

As I have defined the brightness of an object to the eye of an observer at a distance, to be expressed by $\frac{a^2 l}{D^2}$, it will be necessary to answer some objections that may be made to this theory. Optical writers have proved, that an object is equally bright at all distances. It may, therefore, be maintained against me, that since a wall illuminated by the sun will appear equally

bright, at whatsoever distance the observer be placed that views it, the sun also, at the distance of Saturn, or still farther from us, must be as bright as it is in its present situation. Nay, it may be urged, that in a telescope, the different distance of stars can be of no account with regard to their brightness, and that we must consequently be able to see stars which are many thousands of times farther than Sirius from us; in short, that a star must be infinitely distant not to be seen any longer.

Now, objections such as these, which seem to be the immediate consequence of what has been demonstrated by mathematicians, and which yet apparently contradict what I assert in this paper, deserve to be thoroughly answered.

It may be remembered, that I have distinguished brightness into three different sorts.* Two of these, which have been discriminated by *intrinsic* and *absolute* brightness, are, in common language, left without distinction. In order to shew that they are so, I might bring a variety of examples from common conversation; but, taking this for granted, it may be shewn that all the objections I have brought against my theory have their foundation in this ambiguity.

The demonstrations of opticians, with regard to what I call *intrinsic* brightness, will not oppose what I affirm of *absolute* brightness; and I shall have nothing farther to do than to shew that what mathematicians have said, must be understood to refer entirely to the intrinsic brightness, or illumination of the picture of objects on the retina of the eye: from which it will clearly follow, that their doctrine and mine are perfectly reconcileable; and that they can be at variance only when the ambiguity of the word brightness is overlooked, and objections,

* See page 52.

such as I have made, are raised, where the word brightness is used as *absolute*, when we should have kept it to the only meaning it can bear in the mathematicians' theorem.

The first objection I have mentioned is, that the sun, to an observer on Saturn, must be as bright as it is here on earth. Now by this cannot be meant, that an inhabitant standing on the planet Saturn, and looking at the sun, should *absolutely* receive as much light from it as one on earth receives when he sees it; for this would be contrary to the well known decrease of light at various distances. The objection, therefore, can only go to assert, that the picture of the sun, on the retina of the Saturnian observer, is as *intensely* illuminated as that on the retina of the terrestrial astronomer. To this I perfectly agree. But let those who would go farther, and say that therefore the sun is *absolutely* as bright to the one as to the other, remember that the sun on Saturn appears to be a hundred times less than on the earth; and that consequently, though it may there be *intrinsically* as bright, it must here be *absolutely** an hundred times brighter.

The next objection I have to consider, relates to the fixed stars. What has been shewn in the preceding paragraph, with regard to the sun, is so intirely applicable to the stars, that it will be very easy to place this point also in its proper light. As I have assented to the demonstration of opticians with regard to the brightness of the sun, when seen at the distance of Saturn, provided the meaning of this word be kept to the *intrinsic* illumination of the picture on the retina of an observer, I can have no hesitation to allow that the same will hold good with a star placed at any assignable distance. But I must repeat, that

* See the definition of *absolute* brightness, page 52.

the light we can receive from stars is truly expressed by $\frac{a^2 l}{D^2}$; and that therefore their absolute brightness must vary in the inverse ratio of the squares of their distances. Hence I am authorised to conclude, and observation abundantly confirms it, that stars cannot be seen by the naked eye, when they are more than seven or eight times farther from us than Sirius; and that they become, comparatively speaking, very soon invisible with our best instruments. It will be shewn hereafter, that the visibility of stars depends on the penetrating power of telescopes, which, I must repeat, falls indeed very short of shewing stars that are many thousands of times farther from us than Sirius; much less can we ever hope to see stars that are all but infinitely distant.

If now it be admitted that the expressions we have laid down are such as agree with well known facts, we may proceed to vision at a distance; and first with respect to the naked eye.

Here the power of penetrating into space, is not only confined by nature, but is moreover occasionally limited by the failure in brightness of luminous objects. Let us see whether astronomical observations, assisted by mathematical reasoning, can give us some idea of the general extent of natural vision. Among the reflecting luminous objects, our penetrating powers are sufficiently ascertained. From the moon we may step to Venus, to Mercury, to Mars, to Jupiter, to Saturn, and last of all to the Georgian planet. An object seen by reflected light at a greater distance than this, it has never been allowed us to perceive; and it is indeed much to be admired, that we should

see borrowed illumination to the amazing distance of more than 18 hundred millions of miles; especially when that light, in coming from the sun to the planet, has to pass through an equal space, before it can be reflected, whereby it must be so enfeebled as to be above 368 times less intense on that planet than it is with us, and when probably not more than one-third part of that light can be thrown back from its disk.*

The range of natural vision with self-luminous objects, is incomparably more extended, but less accurately to be ascertained. From our brightest luminary, the sun, we pass immediately to very distant objects; for, Sirius, Arcturus, and the rest of the stars of the first magnitude, are probably those that come next; and what their distance may be, it is well known, can only be calculated imperfectly from the doctrine of parallaxes, which places the nearest of them at least 412530 times farther from us than the sun.

In order to take a second step forwards, we must enter into some preliminary considerations, which cannot but be attended with considerable uncertainty. The general supposition, that stars, at least those which seem to be promiscuously scattered, are probably one with another of a certain magnitude, being admitted, it has already been shewn in a former Paper,† that after a certain number of stars of the first magnitude have been arranged about the sun, a farther distant set will come in for the second place. The situation of these may be taken to be, one with another, at about double the distance of the former from us.

* According to Mr. BOUGUER, the surface of the moon absorbs about two-thirds of the light it receives from the sun. See *Traité d'Optique*, page 122.

† Phil. Trans. for the year 1796, page 166, 167, 168.

By directing our view to them, and thus penetrating one step farther into space, these stars of the second magnitude furnish us with an experiment that shews what phænomena will take place, when we receive the illumination of two very remote objects, equally bright in themselves, whereof one is at double the distance of the other. The expression for the brightness of such objects, at all distances, and with any aperture of the iris, according to our foregoing notation, will be $\frac{a^2 l}{D^2}$; and a method of reducing this to an experimental investigation will be as follows.

Let us admit that α Cygni, β Tauri, and others, are stars of the second magnitude, such as are here to be considered. We know, that in looking at them and the former, the aperture of the iris will probably undergo no change; since the difference in brightness, between Sirius, Arcturus, α Cygni, and β Tauri, does not seem to affect the eye so as to require any alteration in the dimensions of the iris; a, therefore becomes a given quantity, and may be left out. Admitting also, that the latter of these stars are probably at double the distance of the former, we have D^2 in one case four times that of the other; and the two expressions for the brightness of the stars, will be l for those of the first magnitude, and $\frac{1}{4}l$ for those of the second.

The quantities being thus prepared, what I mean to suggest by an experiment is, that since sensations, by their nature, will not admit of being halved or quartered, we come thus to know by inspection what phænomenon will be produced by the fourth part of the light of a star of the first magnitude. In this sense, I think we must take it for granted, that a certain idea of brightness, attached to the stars which are generally denominated to

be of the second magnitude, may be added to our experimental knowledge; for, by this means, we are informed what we are to understand by the expressions $\frac{a^2 l}{\odot^2}$, $\frac{a^2 l}{\text{Sirius}^2}$, $\frac{a^2 l}{\beta\,\text{Tauri}^2}$.* We cannot wonder at the immense difference between the brightness of the sun and that of Sirius; since the two first expressions, when properly resolved, give us a ratio of brightness of more than 170 thousand millions to one; whereas the two latter, as has been shewn, give only a ratio of four to one.

What has been said will carry us, with very little addition, to the end of our unassisted power of vision to penetrate into space. We can have no other guide to lead us a third step than the same beforementioned hypothesis; in consequence of which, however, it must be acknowledged to be sufficiently probable, that the stars of the third magnitude may be placed about three times as far from us as those of the first. It has been seen, by my remarks on the comparative brightness of the stars, that I place no reliance on the classification of them into magnitudes;† but, in the present instance, where the question is not to ascertain the precise brightness of any one star, it is quite sufficient to know that the number of the stars of the first three different magnitudes, or different brightnesses, answers, in a general way, sufficiently well to a supposed equally distant arrangement of a first, second, and third set of stars about the sun. Our third step forwards into space, may therefore very properly be said to fall on the pole-star, on γ Cygni, ε Bootis, and all those of the same order.

* The names of the objects $\odot$, Sirius, β Tauri, are here used to express their distance from us.

† Phil. Trans. for the year 1796, page 168, 169.

As the difference, between these and the stars of the preceding order, is much less striking than that between the stars of the first and second magnitude, we also find that the expressions $\frac{a^2 l}{\beta \text{ Tauri}|^2}$, and $\frac{a^2 l}{\text{Polaris}|^2}$, are not in the high ratio of 4 to 1, but only as 9 to 4, or $2\frac{1}{4}$ to 1.

Without tracing the brightness of the stars through any farther steps, I shall only remark, that the diminution of the ratios of brightness of the stars of the 4th, 5th, 6th, and 7th magnitude, seems to answer to their mathematical expressions, as well as, from the first steps we have taken, can possibly be imagined. The calculated ratio, for instance, of the brightness of a star of the 6th magnitude, to that of one of the 7th, is but little more than $1\frac{1}{3}$ to 1; but still we find by experience, that the eye can very conveniently perceive it. At the same time, the faintness of the stars of the 7th magnitude, which require the finest nights, and the best common eyes to be perceived, gives us little room to believe that we can penetrate much farther into space, with objects of no greater brightness than stars.

But, since it may be justly observed, that in the foregoing estimation of the proportional distance of the stars, a considerable uncertainty must remain, we ought to make a proper allowance for it; and, in order to see to what extent this should go, we must make use of the experimental sensations of the ratios of brightness we have now acquired, in going step by step forward: for, numerical ratios of brightness, and sensations of them, as has been noticed before, are very different things. And since, from the foregoing considerations, it may be concluded, that as far as the 6th, 7th, or 8th magnitude, there

ought to be a visible general difference between stars of one order and that of the next following, I think, from the faintness of the stars of the 7th magnitude, we are authorized to conclude, that no star, eight, nine, or at most ten times as far from us as Sirius, can possibly be perceived by the natural eye.

The boundaries of vision, however, are not confined to single stars. Where the light of these falls short, the united lustre of sidereal systems will still be perceived. In clear nights, for instance, we may see a whitish patch in the sword-handle of Perseus,* which contains small stars of various sizes, as may be ascertained by a telescope of a moderate power of penetrating into space. We easily see the united lustre of them, though the light of no one of the single stars could have affected the unassisted eye.

Considerably beyond the distance of the former must be the cluster discovered by Mr. MESSIER, in 1764; north following H Geminorum. It contains stars much smaller than those of the former cluster; and a telescope should have a considerable penetrating power, to ascertain their brightness properly, such as my common 10-feet reflector. The night should be clear, in order to see it well with the naked eye, and it will then appear in the shape of a small nebula.

Still farther from us must be the nebula between η and ζ Herculis, discovered by Dr. HALLEY, in 1714. The stars of it are so small that it has been called a Nebula;† and has been regarded as such, till my instruments of high penetrating

* See the catalogue of a second thousand of new nebulæ and clusters of stars, VI. 33, 34. Phil. Trans. Vol. LXXIX. page 251.

† In the *Connoissance des Temps* for 1783, No. 13, it is described as a nebula without stars.

powers were applied to it. It requires a very clear night, and the absence of the moon, to see it with the natural eye.

Perhaps, among the farthest objects that can make an impression on the eye, when not assisted by telescopes, may be reckoned the nebula in the girdle of Andromeda, discovered by SIMON MARIUS, in 1612. It is however not difficult to perceive it, in a clear night, on account of its great extent.

From the powers of penetrating into space by natural vision, we proceed now to that of telescopes.

It has been shewn, that brightness, or light, is to the naked eye truly represented by $\frac{a^2 l}{D^2}$; in a telescope, therefore, the light admitted will be expressed by $\frac{A^2 l}{D^2}$. Hence it would follow, that the artificial power of penetrating into space should be to the natural one as A to a. But this proportion must be corrected by the practical deficiency in light reflected by mirrors, or transmitted through glasses; and it will in a great measure depend on the circumstances of the workmanship, materials, and construction of the telescope, how much loss of light there will be sustained.

In order to come to some determination on this subject, I made many experiments with plain mirrors, polished like my large ones, and of the same composition of metal. The method I pursued was that proposed by Mr. BOUGUER, in his *Traité d'Optique*, page 16, fig. 3.; but I brought the mirror, during the trial, as close to the line connecting the two objects as possible, in order to render the reflected rays nearly perpendicular.

The result was, that out of 100 thousand incident rays,

67262 were returned; and therefore, if a double reflection takes place, only 45242 will be returned.

Before this light can reach the eye, it will suffer some loss in passing through the eye glass; and the amount of this I ascertained, by taking a highly polished plain glass, of nearly the usual thickness of optical glasses of small focal lengths. Then, by the method of the same author, page 21, fig. 5. I found, that out of 100 thousand incident rays, 94825 were transmitted through the glass. Hence, if two lenses be used, 89918; and, with three lenses, 85265 rays will be transmitted to the eye.

Then, by compounding, we shall have, in a telescope of my construction with one reflection, 63796 rays, out of 100 thousand, come to the eye. In the NEWTONIAN form, with a single eye lens, 42901; and, with a double eye glass 40681 will remain for vision.

There must always remain a considerable uncertainty in the quantities here assigned; as a newly polished mirror, or one in high preservation, will give more light than another that has not those advantages. The quality of metal also will make some difference; but, if it should appear by experiments, that the metals or glasses in use will yield more or less light than here assigned, it is to be understood that the corrections must be made accordingly.

We proceed now to find a proper expression for the power of penetrating into space, that we may be enabled to compare its effects, in different telescopes, with that of the natural eye.

Since then the brightness of luminous objects is inversely as the squares of the distances, it follows, that the penetrating power must be as the square roots of the light received by the eye.

In natural vision, therefore, this power is truly expressed by $\sqrt{a^2 l}$; and, since we have now also obtained a proper correction x, we must apply it to the incident light with telescopes.

In the NEWTONIAN and other constructions where two specula are used, there will also be some loss of light on account of the interposition of the small speculum; therefore, putting b for its diameter, we have $\overline{A^2 - b^2}$ for the real incident light. This being corrected as above, will give the general expression $\sqrt{xl \times \overline{A^2 - b^2}}$ for the same power in telescopes. But here we are to take notice, that in refractors, and in telescopes with one reflection, b will be $= o$, and therefore is to be left out.

Then, if we put natural light $l = 1$, and divide by a, we have the general form $\frac{\sqrt{x \cdot \overline{A^2 - b^2}}}{a}$ for the penetrating power of all sorts of telescopes, compared to that of the natural eye as a standard, according to any supposed aperture of the iris, and proportion of light returned by reflection, or transmitted by refraction.

In the following investigation we shall suppose $a = 2$ tenths of an inch, as being perhaps nearly the general opening of the iris, in star-light nights, when the eye has been some moderate time in the dark. The value of the corrections for loss of light will stand as has been given before.

We may now proceed to determine the powers of the instruments that have been used in my astronomical observations; but, as this subject will be best explained by a report of the

observations themselves, I shall select a series of them for that purpose, and relate them in the order which will be most illustrating.

First, with regard to the eye, it is certain that its power, like all our other faculties, is limited by nature, and is regulated by the permanent brightness of objects; as has been shewn already, when its extent with reflected light was compared to its exertion on self-luminous objects. It is further limited on borrowed light, by the occasional state of illumination; for, when that becomes defective at any time, the power of the eye will then be contracted into a narrower compass; an instance of which is the following.

In the year 1776, when I had erected a telescope of 20 feet focal length, of the NEWTONIAN construction, one of its effects by trial was, that when towards evening, on account of darkness, the natural eye could not penetrate far into space, the telescope possessed that power sufficiently to shew, by the dial of a distant church steeple, what o'clock it was, notwithstanding the naked eye could no longer see the steeple itself. Here I only speak of the penetrating power; for, though it might require magnifying power to see the figures on the dial, it could require none to see the steeple. Now the aperture of the telescope being 12 inches, and the construction of the NEWTONIAN form, its penetrating power, when calculated according to the given formula, will be $\frac{\sqrt{,429 \times 120^3 - 15^2}}{2} = 38{,}99$. *A*, *b*, and *a*, being all expressed in tenths of an inch.*

* I have given the figures, in all the following equations of the calculated penetrating powers, in order to shew the constructions of my instruments to those who may wish to be acquainted with them.

From the result of this computation it appears, that the circumstance of seeing so well, in the dusk of the evening, may be easily accounted for, by a power of this telescope to penetrate 89 times farther into space than the natural eye could reach, with objects so faintly illuminated.

This observation completely refutes an objection to telescopic vision, that may be drawn from what has also been demonstrated by optical writers; namely, that no telescope can shew an object brighter than it is to the naked eye. For, in order to reconcile this optical theory with experience, I have only to say, that the objection is intirely founded on the same ambiguity of the word brightness that has before been detected. It is perfectly true, that the *intrinsic* illumination of the picture on the retina, which is made by a telescope, cannot exceed that of natural vision; but the *absolute* brightness of the magnified picture by which telescopic vision is performed, must exceed that of the picture in natural vision, in the same ratio in which the area of the magnified picture exceeds that of the natural one; supposing the *intrinsic* brightness of both pictures to be the same. In our present instance, the steeple and clock-dial were rendered visible by the increased absolute brightness of the object, which in natural vision was 15 hundred times inferior to what it was in the telescope. And this establishes beyond a doubt, that telescopic vision is performed by the absolute brightness of objects; for, in the present case, I find by computation, that the *intrinsic* brightness, so far from being equal in the telescope to that of natural vision, was inferior to it in the ratio of three to seven.

The distinction between magnifying power, and a power of penetrating into space, could not but be felt long ago, though

its theory has not been inquired into. This undoubtedly gave rise to the invention of those very useful short telescopes called night-glasses. When the darkness of the evening curtails the natural penetrating power, they come in very seasonably, to the relief of mariners that are on the look out for objects which it is their interest to discover. Night-glasses, such as they are now generally made, will have a power of penetrating six or seven times farther into space than the natural eye. For, by the construction of the double eye-glass, these telescopes will magnify 7 or 8 times; and the object glass being $2\frac{1}{2}$ inches in diameter, the breadth of the optic pencil will be $3\frac{1}{7}$ or $3\frac{1}{7}$ tenths of an inch. As this cannot enter the eye, on a supposition of an opening of the iris of 2 tenths, we are obliged to increase the value of a, in order to make the telescope have its proper effect. Now, whether nature will admit of such an enlargement becomes an object of experiment; but, at all events, a cannot be assumed less than $\frac{A}{m}$. Then, if x be taken as has been determined for three refractions, we shall have $\frac{\sqrt{.853 \times 25^2}}{a} = 6{,}46$ or $7{,}39$.

Soon after the discovery of the Georgian planet, a very celebrated observer of the heavens, who has added considerably to our number of telescopic comets and nebulæ, expressed his wish, in a letter to me, to know by what method I had been led to suspect this object not to be a star, like others of the same appearance. I have no doubt but that the instrument through which this astronomer generally looked out for comets, had a penetrating power much more than sufficient to shew the new planet, since even the natural eye will reach it. But here we have an instance of the great difference in the effect of the two sorts of powers of telescopes; for, on account of the smallness

of the planet, a different sort of power, namely, that of magnifying, was required; and, about the time of its discovery, I had been remarkably attentive to an improvement of this power, as I happened to be then much in want of it for my very close double stars.*

On examining the nebulæ which had been discovered by many celebrated authors, and comparing my observations with the account of them in the *Connoissance des Temps* for 1783, I found that most of those which I could not resolve into stars with instruments of a small penetrating power, were easily resolved with telescopes of a higher power of this sort; and, that the effect was not owing to the magnifying power I used upon these occasions, will fully appear from the observations; for, when the closeness of the stars was such as to require a considerable degree of magnifying as well as penetrating power, it always appeared plainly, that the instrument which had the highest penetrating power resolved them best, provided it had as much of the other power as was required for the purpose.

Sept. 20, 1783, I viewed the nebula between FLAMSTEED's 99th and 105th Piscium, discovered by Mr. MECHAIN, in 1780.

"It is not visible in the finder of my 7-feet telescope; but "that of my 20-feet shews it."

Oct. 28, 1784, I viewed the same object with the 7-feet telescope.

"It is extremely faint. With a magnifying power of 120, it "seems to be a collection of very small stars: I see many of "them."

* Magnifying powers of 460, 625, 932, 1159, 1504, 2010, 2398, 3168, 4294, 5489, 6450, 6652, were used upon ι Bootis, γ Leonis, α Lyræ, &c. See Cat. of double stars, Phil. Trans. Vol. LXXII. page 115, and 147; and Vol. LXXV. page 48.

At the time of these observations, my 7-feet telescope had only a common finder, with an aperture of the object glass of about $\frac{3}{4}$ of an inch in diameter, and a single eye-lens; therefore its penetrating power was $\frac{\sqrt{,899 \times 7,5^2}}{2} = 3,56$. The finder of the 20-feet instrument, being achromatic, had an object glass 1,17 inch in diameter; its penetrating power, therefore, was $\frac{\sqrt{,85 \times 11,7^2}}{2} = 4,50$.

Now, that one of them shewed the nebula and not the other, can only be ascribed to space-penetrating power, as both instruments were equal in magnifying power, and that so low as not to require an achromatic object glass to render the image sufficiently distinct.

The 7-feet reflector evidently reached the stars of the nebula; but its penetrating and magnifying powers are very considerable, as will be shewn presently.

July 30, 1783, I viewed the nebula south preceding FLAMSTEED's 24 Aquarii, discovered by Mr. MARALDI, in 1746.

" In the small *sweeper*,* this nebula appears like a telescopic " comet."

Oct. 27, 1794. The same nebula with a 7-feet reflector.

* The small sweeper is a NEWTONIAN reflector, of 2 feet focal length; and, with an aperture of 4,2 inches, has only a magnifying power of 24, and a field of view 2° 12′. Its distinctness is so perfect, that it will shew letters at a moderate distance, with a magnifying power of 2000; and its movements are so convenient, that the eye remains at rest while the instrument makes a sweep from the horizon to the zenith.

A large one of the same construction has an aperture of 9,2 inches, with a focal length of 5 feet 3 inches. It is also charged low enough for the eye to take in the whole optic pencil; and its penetrating power, with a double eye glass, is $\frac{\sqrt{,41 \times 92^2 - 21^2}}{2} = 28,57$.

"I can see that it is a cluster of stars, many of them being "visible."

If we compare the penetrating power of the two instruments, we find that we have in the first $\frac{\sqrt{.41\times 42^2-12^2}}{2} = 12{,}84$; and in the latter $\frac{\sqrt{.43\times 63^2-12^2}}{2} = 20{,}25$. However, the magnifying power was partly concerned in this instance; for, in the *sweeper* it was not sufficient to separate the stars properly.

March 4, 1783. With a 7-feet reflector, I viewed the nebula near the 5th Serpentis, discovered by Mr. MESSIER, in 1764.

"It has several stars in it; they are however so small that I "can but just perceive some, and suspect others."

May 31, 1783. The same nebula with a 10-feet reflector; penetrating power $\frac{\sqrt{.43\times 89^2-16^2}}{2} = 28{,}67$.

"With a magnifying power of 250, it is all resolved into "stars: they are very close, and the appearance is beautiful. "With 600, perfectly resolved. There is a considerable star not "far from the middle; another not far from one side, but out "of the cluster; another pretty bright one; and a great number "of small ones."

Here we have a case where the penetrating power of 20 fell short, when 29 resolved the nebula completely. This object requires also great magnifying power to shew the stars of it well; but that power had before been tried, in the 7-feet, as far as 460, without success, and could only give an indication of its being composed of stars; whereas the lower magnifying power of 250, with a greater penetrating power, in the 10-feet instrument, resolved the whole nebula into stars.

May 3, 1783. I viewed the nebula between η and ζ Ophiuchi, discovered by Mr. MESSIER, in 1764.

"With a 10-feet reflector, and a magnifying power of 250, "I see several stars in it, and make no doubt a higher power, "and more light, will resolve it all into stars. This seems to be "a good nebula for the purpose of establishing the connection "between nebulæ and clusters of stars in general."

June 18, 1784. The same nebula viewed with a large NEWTONIAN 20-feet reflector; penetrating power $\frac{\sqrt{,43\times 188^2-21^2}}{2}$ $=61,18$; and a magnifying power of 157.

"A very large and very bright cluster of excessively com- "pressed stars. The stars are but just visible, and are of une- "qual magnitudes: the large stars are red; and the cluster is "a miniature of that near FLAMSTEED's 42d Comæ Berenices. "RA 17^h 6′ 32″; PD 108° 18′."

Here, a penetrating power of 29, with a magnifying power of 250, would barely shew a few stars; when, in the other instrument, a power 61 of the first sort, and only 157 of the latter, shewed them completely well.

July 4, 1783. I viewed the nebula between FLAMSTEED's 25 and 26 Sagittarii, discovered by ABRAHAM IHLE, in 1665.

"With a small 20-feet NEWTONIAN telescope, power 2[illegible] "it is all resolved into stars, that are very small and clos[illegible] "There must be some hundreds of them. With 350, I see the "stars very plainly; but the nebula is too low in this latitude "for such a power."

July 12, 1784. I viewed the same nebula with a large 20-feet NEWTONIAN reflector; power 157.

"A most beautiful extensive cluster of stars, of various mag- "nitudes, very compressed in the middle, and about 8′ in

" diameter, besides the scattered ones, which do more than fill " the extent of the field of view:* the large stars are red; the " small ones are pale red. RA 18h 23′ 39″; PD 114° 7′."

The penetrating power of the first instrument was 39, that of the latter 61; but, from the observations, it is plain how much superior the effect of the latter was to that of the former, notwithstanding the magnifying power was so much in favour of the instrument with the small penetrating power.

July 30, 1788. With a small 20-feet NEWTONIAN reflector, I viewed the nebula in the hand of Serpentarius, discovered by Mr. MESSIER, in 1764.

" With a power of 200, I see it consists of stars. They are " better visible with 300. With 600, they are too obscure to be " distinguished, though the appearance of stars is still preserved. " This seems to be one of the most difficult objects to be " resolved. With me, there is not a doubt remaining; but " another person, in order to form a judgment, ought previously " to go through all the several gradations of nebulæ which I " have resolved into stars."

May 25, 1791. I viewed the same nebula with a 20-feet reflector of my construction, having a penetrating power of $\frac{\sqrt{.64 \times 188}^{\circ}}{2} = 75{,}08$.

" With a magnifying power of 157, it appears extremely " bright, round, and easily resolvable. With 300, I can see the " stars. It resembles the cluster of stars taken at 16h 43′ 40″,†

* This field, by the passage of an equatorial star, was 15′ 3″.

† The object referred to is No. 10. of the *Connoissance des Temps* for 1783, called " *Nebuleuse sans etoiles.*" My description of it is, " A very beautiful, and extremely " compressed, cluster of stars: the most compressed part about 3 or 4′ in diameter. " RA 16h 46′ 2″; PD 93° 46′."

" which probably would put on the same appearance as this, " if it were at a distance half as far again as it is. RA 17^h 26′ " 19″; PD 93° 10′."

Here we may compare two observations; one taken with the penetrating power of 39, the other with 75; and, although the former instrument had far the advantage in magnifying power, the latter certainly gave a more complete view of the object.

The 20-feet reflector having been changed from the NEWTONIAN form to my present one, I had a very striking instance of the great advantage of the increased penetrating power, in the discovery of the Georgian satellites. The improvement, by laying aside the small mirror, was from 61 to 75; and, whereas the former was not sufficient to reach these faint objects, the latter shewed them perfectly well.

March 14, 1798. I viewed the Georgian planet with a new 25-feet reflector. Its penetrating power is $\frac{\sqrt{.64 \times 240^2}}{2} = 95{,}85$; and, having just before also viewed it with my 20-feet instrument, I found, that with an equal magnifying power of 300, the 25-feet telescope had considerably the advantage of the former.

Feb. 24, 1786. I viewed the nebula near FLAMSTEED's 5th Serpentis, which has been mentioned before, with my 20-feet reflector; magnifying power 157.

" The most beautiful extremely compressed cluster of small " stars; the greatest part of them gathered together into one " brilliant nucleus, evidently consisting of stars, surrounded " with many detached gathering stars of the same size and " colour. RA 15^h 7′ 12″; PD 87° 8′."

May 27, 1791. I viewed the same object with my 40-feet

telescope; penetrating power $\frac{\sqrt{,64\times 4801^2}}{2} = 191{,}69$; magnifying power 370.

"A beautiful cluster of stars. I counted about 200 of them. "The middle of it is so compressed that it is impossible to dis- "tinguish the stars."

Here it appears, that the superior penetrating power of the 40-feet telescope enabled me even to count the stars of this nebula. It is also to be noticed, that the object did not strike me as uncommonly beautiful; because, with much more than double the penetrating, and also more than double the magnifying power, the stars could not appear so compressed and small as in the 20-feet instrument: this, very naturally, must give it more the resemblance of a coarser cluster of stars, such as I had been in the habit of seeing frequently.

The 40-feet telescope was originally intended to have been of the NEWTONIAN construction; but, in the year 1787, when I was experimentally assured of the vast importance of a power to penetrate into space, I laid aside the work of the small mirror, which was then in hand, and completed the instrument in its present form.

"Oct. 10, 1791. I saw the 4th satellite and the ring of "Saturn, in the 40-feet speculum, without an eye glass."

The magnifying power on that occasion could not exceed 60 or 70; but the great penetrating power made full amends for the lowness of the former; notwithstanding the greatest part of it must have been lost for want of a greater opening of the iris, which could not take in the whole pencil of rays, for this could not be less than 7 or 8 tenths of an inch.

Among other instances of the superior effects of penetration into space, I should mention the discovery of an additional 6th satellite of Saturn, on the 28th of August, 1789; and of a 7th, on the 11th of September, in the same year; which were first pointed out by this instrument. It is true that both satellites are within the reach of the 20-feet telescope; but it should be remembered, that when an object is once discovered by a superior power, an inferior one will suffice to see it afterwards. I need not add, that neither the 7 nor 10-feet telescopes will reach them; their powers, 20 and 29, are not sufficient to penetrate to such distant objects, when the brightness of them is not more than that of these satellites. It is also evident, that the failure in these latter instruments, arises not from want of magnifying power; as either of them has much more than sufficient for the purpose.

Nov. 5, 1791. I viewed Saturn with the 20 and 40-feet telescopes.

"20-feet. The 5th satellite of Saturn is very small. The 1st, "2d, 3d, 4th, 5th, and the new 6th satellite, are in their cal-"culated places."

"40-feet. I see the new 6th satellite much better with this "instrument than with the 20-feet. The 5th is also much larger "here than in the 20-feet; in which it was nearly the same size "as a small fixed star, but here it is considerably larger than "that star."

Here the superior penetrating power of the 40-feet telescope shewed itself on the 6th satellite of Saturn, which is a very faint object; as it had also a considerable advantage in magnifying power, the disk of the 5th satellite appeared larger than in the 20-feet. But the small star, which may be said to be beyond

the reach of magnifying power, could only profit by the superiority of the other power.

Nov. 21, 1791. 40-feet reflector; power 370.

" The black division upon the ring is as dark as the heavens " about Saturn, and of the same colour."

" The shadow of the body of Saturn is visible upon the ring, " on the following side; its colour is very different from that " of the dark division. The 5th satellite is less than the 3d; it " is even less than the 2d."

20-feet reflector; power 300.

" The 3d satellite seems to be smaller than it was the last night " but one. The 4th satellite seems to be larger than it was the " 19th. This telescope shews the satellites not nearly so well " as the 40-feet."

Here, the magnifying power being nearly alike, the superiority of the 40-feet telescope must be ascribed to its penetrating power.

The different nature of the two powers above mentioned being thus evidently established, I must now remark, that, in some respects, they even interfere with each other; a few instances of which I shall give.

August 24, 1783. I viewed the nebula north preceding FLAMSTEED's 1 Trianguli, discovered by Mr. MESSIER, in 1764.

" 7-feet reflector; power 57. There is a suspicion that the " nebula consists of exceedingly small stars. With this low

" power it has a nebulous appearance; and it vanishes when I " put on the higher magnifying powers of 278 and 460."

Oct. 28, 1794. I viewed the same nebula with a 7-feet reflector.

" It is large, but very faint. With 120, it seems to be com- " posed of stars, and I think I see several of them; but it will " bear no magnifying power."

In this experiment, magnifying power was evidently injurious to penetrating power. I do not account for this upon the principle that by magnifying we make an object less bright; for, when opticians have also demonstrated that brightness is diminished by magnifying, it must again be understood as relating only to the *intrinsic* brightness of the magnified picture; its absolute brightness, which is the only one that concerns us at present, must always remain the same.* The real explanation of the fact, I take to be, that while the light collected is employed in magnifying the object, it cannot be exerted in giving penetrating power.

* This may be proved thus. The mean intrinsic brightness, or rather illumination, of a point of the picture on the retina, will be *all the light that falls on the picture, divided by the number of its points*; or $C = \frac{l}{N}$. Now, since with a greater magnifying power m, the number of points N increases as the squares of the power, the expression for the intrinsic brightness $\frac{l}{N}$, will decrease in the same ratio; and it will consequently be in general $N \propto m^2$, and $\frac{l}{N}$ or $C \propto \frac{1}{m^2}$; that is, by compounding $CN \propto \frac{m^2}{m^2} = l = 1$; or absolute brightness a given quantity. M. Bouguer has carefully distinguished intrinsic and absolute brightness, when he speaks of the quantity of light reflected from a wall, at different distances. *Traité d'Optique*, page 39, and 40.

June 18, 1799. I viewed the planet Venus with a 10-feet reflector.

"Its light is so vivid that it does not require, nor will it bear, "a penetrating power of 29, neither with a low nor with a "high magnifying power."

This is not owing to the least imperfection in the mirror, which is truly parabolical, and shews, with all its aperture open, and a magnifying power of 600, the double star γ Leonis in the greatest perfection.

"It shewed Venus, perfectly well defined, with a penetrating "power as low as 14, and a magnifying power of 400, or 600."

Here, penetrating power was injurious to magnifying power; and that it necessarily must be so, when carried to a high pitch, is evident; for, by enlarging the aperture of the telescope, we increase the evil that attends magnifying, which is, that we cannot magnify the object without magnifying the medium. Now, since the air is very seldom of so homogeneous a disposition as to admit to be magnified highly, it follows that we must meet with impurities and obstructions, in proportion to its quantity. But the contents of the columns of air through which we look at the heavens by telescopes, being of equal lengths, must be as their bases, that is, as the squares of the apertures of the telescopes; and this is in a much higher ratio than that of the increase of the power of penetrating into space. From my long experience in these matters, I am led to apprehend, that the highest power of magnifying may possibly not exceed the reach of a 20 or 25-feet telescope; or may even lie in a less compass than either. However, in beautiful nights, when the outside of our telescopes is dropping with moisture discharged from the atmosphere, there are now and then favourable

hours, in which it is hardly possible to put a limit to magnifying power. But such valuable opportunities are extremely scarce; and, with large instruments, it will always be lost labour to observe at other times.

As I have hinted at the natural limits of magnifying power, I shall venture also to extend my surmises to those of penetrating power. There seems to be room for a considerable increase in this branch of the telescope; and, as the penetrating power of my 40-feet reflector already goes to 191,69, there can hardly be any doubt but that it might be carried to 500, and probably not much farther. The natural limit seems to be an equation between the faintest star that can be made visible, by any means, and the united brilliancy of star-light. For, as the light of the heavens, in clear nights, is already very considerable in my large telescope, it must in the end be so increased, by enlarging the penetrating power, as to become a balance to the light of all objects that are so remote as not to exceed in brightness the general light of the heavens. Now, if P be put for penetrating power, we have $\sqrt{\frac{P^2 a^3}{x}} = A =$ 10 feet 5,2 inches for an aperture of a reflector, on my construction, that would have such a power of 500.

But, to return to our subject; from what has been said before, we may conclude, that objects are viewed in their greatest perfection, when, in penetrating space, the magnifying power is so low as only to be sufficient to shew the object well; and when, in magnifying objects, by way of examining them minutely, the space-penetrating power is no higher that what will suffice for the purpose; for, in the use of either power, the injudicious overcharge of the other, will prove hurtful to perfect vision.

It is remarkable that, from very different principles, I have formerly determined the length of the visual ray of my 20-feet telescope upon the stars of the milky way, so as to agree nearly with the calculations that have been given.* The extent of what I then figuratively called my sounding line, and what now appears to answer to the power of penetrating into space, was shewn to be not less than 415, 461, and 497 times the distance of Sirius from the sun. We now have calculated that my telescope, in the NEWTONIAN form, at the time when the paper on the Construction of the Heavens was written, possessed a power of penetration, which exceeded that of natural vision 61,18 times; and, as we have also shewn, that stars at 8, 9, or at most 10 times the distance of Sirius, must become invisible to the eye, we may safely conclude, that no single star, above 489, 551, or at most 612 times as far as Sirius, can any longer be seen in this telescope. Now, the greatest length of the former visual ray, 497, agrees nearly with the lowest of these present numbers, 489; and the higher ones are all in favour of the former computation; for that ray, though taken from what was perhaps not far from its greatest extent, might possibly have reached to some distance beyond the apparent bounds of the milky way: but, if there had been any considerable difference in these determinations, we should remember that some of the data by which I have now calculated are only assumed. For instance, if the opening of the iris, when we look at a star of the 7th magnitude, should be only one-tenth of an inch and a half, instead of two, then a, in our formula, will be $= 1{,}5$; which, when resolved,

* Phil. Trans. Vol. LXXV. page 247, 248.

will give a penetrating power of 81,58: and therefore, on this supposition, our telescope would easily have shewn stars 571 times as far from us as Sirius; and only those at 653, 734, or 816 times the same distance, would have been beyond its reach. My reason for fixing upon two-tenths, rather than a lower quantity, was, that I might not run a risk of over-rating the powers of my instruments. I have it however in contemplation, to determine this quantity experimentally, and perceive already, that the difficulties which attend this subject may be overcome.

It now only remains to shew, how far the penetrating power, 192, of my large reflector, will really reach into space. Then, since this number has been calculated to be in proportion to the standard of natural vision, it follows, that if we admit a star of the 7th magnitude to be visible to the unassisted eye, this telescope will shew stars of the one thousand three hundred and forty-second magnitude.

But, as we did not stop at the single stars above mentioned, when the penetration of the natural eye was to be ascertained, so we must now also call the united lustre of sidereal systems to our aid in stretching forwards into space. Suppose therefore, a cluster of 5000 stars to be at one of those immense distances to which only a 40-feet reflector can reach, and our formula will give us the means of calculating what that may be. For, putting S for the number of stars in the cluster, and D for its distance, we have $\frac{\sqrt{x}\,A^{n}\,S}{a} = D$; * which, on computation,

* $D = 1176547594867867867 9$ miles.

comes out to be above $11\frac{1}{4}$ millions of millions of millions of miles! A number which exceeds the distance of the nearest fixed star, at least three hundred thousand times.

From the above considerations it follows, that the range for observing, with a telescope such as my 40-feet reflector, is indeed very extensive. We have the inside of a sphere to examine, the radius of which is the immense distance just now assigned to be within the reach of the penetration of our instruments, and of which all the celestial objects visible to the eye, put together, form as it were but the kernel, while all the immensity of its thick shell is reserved for the telescope.

It follows, in the next place, that much time must be required for going through so extensive a range. The method of examining the heavens, by sweeping over space, instead of looking merely at places that are known to contain objects, is the only one that can be useful for discoveries.

In order therefore to calculate how long a time it must take to sweep the heavens, as far as they are within the reach of my 40-feet telescope, charged with a magnifying power of 1000, I have had recourse to my journals, to find how many favourable hours we may annually hope for in this climate. It is to be noticed, that the nights must be very clear; the moon absent; no twilight; no haziness; no violent wind; and no sudden change of temperature; then also, short intervals for filling up broken sweeps will occasion delays; and, under all these circumstances, it appears that a year which will afford 90, or at most 100 hours, is to be called very productive.

In the equator, with my 20-feet telescope, I have swept over zones of two degrees, with a power of 157; but, an allowance of 10 minutes in polar distance must be made, for lapping the sweeps over one another where they join.

As the breadth of the zones may be increased towards the poles, the northern hemisphere may be swept in about 40 zones: to these we must add 19 southern zones; then, 59 zones, which, on account of the sweeps lapping over one another about $5'$ of time in right ascension, we must reckon of 25 hours each, will give 1475 hours. And, allowing 100 hours per year, we find that, with the 20-feet telescope, the heavens may be swept in about 14 years and $\frac{3}{4}$.

Now, the time of sweeping with different magnifying powers will be as the squares of the powers; and, putting p and t for the power and time in the 20-feet telescope, and $P = 1000$ for the power in the 40, we shall have $p^2 : t :: P^2 : \frac{t P^2}{p^2} = 59840$. Then, making the same allowance of 100 hours per year, it appears that it will require not less than 598 years, to look with the 40-feet reflector, charged with the abovementioned power, only one single moment into each part of space; and, even then, so much of the southern hemisphere will remain unexplored, as will take up 213 years more to examine.

Slough, near Windsor,
June 20, 1799.

V. *A second Appendix to the improved Solution of a Problem in physical Astronomy, inserted in the Philosophical Transactions for the Year* 1798, *containing some further Remarks, and improved* Formulæ *for computing the Coefficients* A *and* B; *by which the arithmetical Work is considerably shortened and facilitated. By the Rev.* John Hellins, *B. D. F. R. S. and Vicar of* Potter's Pury, *in* Northamptonshire.

Read December 12, 1799.

1. It was shewn, in Art. 9. of the first Appendix, that the common logarithm of the fraction $\frac{1+\sqrt{(1-cc)}}{c}$, when c is expressed in numbers, might be taken out from Taylor's excellent tables, and converted into an hyperbolic logarithm by means of table XXXVII. of Dodson's Calculator; which method of obtaining the H. L. $\frac{1+\sqrt{(1-cc)}}{c}$ is undoubtedly easier and shorter than the more obvious one of first computing the numerical value of that fraction, and then taking out the hyperbolic logarithm corresponding to it from a table. But yet, that method of obtaining the value of α, easy as it is, requires, first, a search in the table for the angle of which c is the sine, and generally a proportion for the fractional parts of a second; then, a division of the degrees, minutes, and seconds contained in that angle, by 2; and, thirdly, another search for the logarithmic tangent of half the angle, and another proportion to find the fractional parts of a second. I was therefore desirous

of finding some easier and shorter method of performing the whole business, without the use of any trigonometrical tables, in which time is required, not only in searching for logarithms, but also in making proportions for the fractional parts of a second; and, after some consideration, I discovered that which I am now to explain.

This method, then, together with some further observations which I have made for facilitating and abridging the work of computing the values of A and B, will make up the contents of this Paper.

2. The H. L. $\frac{1+\sqrt{(1-cc)}}{c}$, which was denoted by α, both in the solution of the problem and in the Appendix, is $=$ H. L. $\frac{2}{c}, -\frac{cc}{2\,2} - \frac{3\,c^4}{2.4\,4} - \frac{3.5\,c^6}{2\,4\,6.6}$,* &c. and if, for the sake of distinction, the Roman letter a be put for H. L. $\frac{2}{c}$, we shall have $\alpha = a - \frac{cc}{4} - \frac{3\,c^4}{32}$, &c. (of which series, the first three terms are sufficient for our present purpose); and this value of α being written for it in the expression $\alpha\left(1 + \frac{3}{8}cc + \frac{3.5}{8.8}c^4\right)$, which occurs in the first theorem in Art. 12. of the first Appendix, we shall have $\left(1 + \frac{3}{8}cc + \frac{3.5}{8.8}c^4\right) \times \left(a - \frac{cc}{4} - \frac{3}{4.8}c^4\right)$; that is, by actual multiplication,

* Since H. L. $\frac{2}{1+\sqrt{(1-cc)}}$ is $= \frac{cc}{2.2} + \frac{3\,c^4}{2.4\,4} + \frac{3.5\,c^6}{2.4.6.6}$, &c. (See Art. 2. of the first Appendix.) the H. L. $\frac{1+\sqrt{(1-cc)}}{2}$ will be $= -\frac{cc}{2.2} - \frac{3\,c^4}{2\,4.4} - \frac{3\,5\,c^6}{2.4.6.6}$, &c. and consequently H. L. $\frac{1+\sqrt{(1-cc)}}{c}$, $\left(= \frac{2}{c} \times \frac{1+\sqrt{(1-cc)}}{2}\right)$ will be $=$ H. L. $\frac{2}{c}, -\frac{cc}{4} - \frac{3\,c^4}{4.8}$, &c.

$$\begin{array}{l} 1 + \frac{3}{8}cc + \frac{3\cdot5}{8\cdot8}c^4 \\ a - \frac{1}{4}cc - \frac{3}{4\cdot8}c^4 \\ \hline a + \frac{3}{8}acc + \frac{3\cdot5}{8\cdot8}ac^4 \\ \quad - \frac{1}{4}cc - \frac{3}{4\cdot8}c^4 \\ \quad - \frac{3}{4\cdot8}c^4 \end{array} \Bigg\} = \begin{cases} a + \frac{3}{8}acc + \frac{3\cdot5}{8\cdot8}ac^4 \\ \quad - \frac{1}{4}cc - \frac{3}{16}c^4. \end{cases}$$

Now the terms $-\frac{1}{4}cc$ and $-\frac{3}{16}c^4$ may very easily be added to the terms fcc and gc^4, *i.e.* to $0{\cdot}1036802\,cc$ and $0{\cdot}0687064\,c^4$, which will then become $-0{\cdot}1463198\,cc$, and $-0{\cdot}1187936\,c^4$; and, by denoting the coefficients of these new terms by the Roman letters $-$ f and $-$ g respectively, the first theorem in the Art. before mentioned, or the value of A, is

$$\frac{1}{\pi(a+b)^{\frac{1}{2}}} \times \begin{cases} \frac{2}{cc} + e - fcc - gc^4 \\ \quad + a + \frac{3}{8}acc + \frac{3\cdot5}{8\cdot8}ac^4. \end{cases}$$

3. The expression $\alpha\left(\frac{3}{4} + \frac{3\cdot5}{4\cdot12}cc + \frac{3\cdot5\cdot21}{4\cdot12\cdot32}c^4\right)$, which occurs in the value of A′, in Art. 12. of the first Appendix, is $=$

$$\begin{array}{l} \frac{3}{4} + \frac{3\cdot5}{4\cdot12}cc + \frac{3\cdot5\cdot21}{4\cdot12\cdot32}c^4 \\ a - \frac{1}{4}cc - \frac{3}{4\cdot8}c^4 \\ \hline \frac{3}{4}a + \frac{3\cdot5}{4\cdot12}acc + \frac{3\cdot5\cdot21}{4\cdot12\cdot32}ac^4 \\ \quad - \frac{3}{16}cc - \frac{3\cdot5}{12\cdot16}c^4 \\ \quad - \frac{9}{8\cdot16}c^4 \end{array} \Bigg\} = \begin{cases} \frac{3}{4}a + \frac{3\cdot5}{4\cdot12}acc + \frac{3\cdot5\,21}{4\cdot12\cdot32}ac^4 \\ \quad - \frac{3}{16}cc - \frac{19}{8\cdot16}c^4. \end{cases}$$

Here again the terms $-\frac{3}{16}cc$ and $-\frac{19}{8\cdot16}c^4$ may very easily be added to the terms icc and kc^4, *i.e.* to $0{\cdot}0551502\,cc$ and

0·0408309 c^4, and we shall have the two new terms — 0·1323498 cc and — 0·1076091 c^4. Let the coefficients of these two new terms be denoted by the Roman letters — i and — k respectively, and the second theorem in Art. 12 of the first Appendix becomes

$$A' = \frac{1}{\pi(a+b)^{\frac{1}{2}}} \times \left\{ \begin{array}{l} \frac{4}{3c^4} + \frac{1}{cc} + b - icc - kc^4 \\ \qquad + \frac{3}{4}a + \frac{3\cdot 5}{4\cdot 12}acc + \frac{3\cdot 5\cdot 21}{4\cdot 12\cdot 32}ac^4. \end{array} \right.$$

4. The product of $\alpha\left(2 + \frac{1}{2}cc + \frac{9}{16}c^4\right)$, which is found in the third theorem of the Art. before referred to, is =

$$\begin{array}{l} 2 + \frac{1}{2}cc + \frac{9}{16}c^4 \\ a - \frac{1}{4}cc - \frac{3}{4\cdot 8}c^4 \\ \hline 2a + \frac{1}{2}acc + \frac{9}{16}ac^4 \\ \quad - \frac{1}{2}cc - \frac{1}{8}c^4 \\ \quad - \frac{3}{16}c^4 \end{array} = \left\{ \begin{array}{l} 2a + \frac{1}{2}acc + \frac{9}{16}ac^4 \\ \quad - \frac{1}{2}cc \quad - \frac{5}{16}c^4. \end{array} \right.$$

Here likewise, the terms $-\frac{1}{2}cc$ and $-\frac{3}{16}c^4$ may be added to 0·3465736 cc and 0·1793226 c^4, which are $= lcc$ and mc^4 respectively; the coefficients of which being denoted by the Roman letters l and m, the third theorem in the Art. before referred to becomes

$$B = \frac{2a}{b}A - \frac{2}{\pi b(a+b)^{\frac{1}{2}}} \times \left\{ \begin{array}{l} p - lcc - mc^4 \\ + 2a + \frac{1}{2}acc + \frac{9}{2\cdot 16}ac^4. \end{array} \right.$$

5. These new forms to which the theorems are now brought, it is evident, are no less convenient, and on examination they will be found no less accurate, than the original ones; and, that the common logarithm of $\frac{2}{c}$, (and consequently the hyperbolic logarithm of it,) is much more easily and expeditiously obtained

than the common logarithm of $\frac{1+\sqrt{(1-cc)}}{c}$, even with the use of TAYLOR's excellent tables, is too obvious to need a description; and therefore it follows, that a computation by these new *formulæ* will be easier and shorter than by those in the first Appendix.

6. But there are still some expedients by which the computations of A, B, *&c.* may be further facilitated and abridged.

It is pretty evident, to any one who contemplates the coefficients of the logarithmic terms in the first three theorems, that the terms $a + \frac{3}{8}acc + \frac{3\cdot5}{8\cdot8}ac^4$, in the first theorem, being once found, the logarithmic terms of the second and third theorems may most easily be derived from them; in consequence of which, the greater part of the time of writing down the logarithms of $\frac{5}{12}$, $\frac{21}{32}$, $\frac{1}{2}$, and $\frac{9}{16}$, of twice writing down the logarithms of acc and ac^4, and of searching in the tables for the numbers corresponding to $\frac{3\cdot5}{4\cdot12}acc$, and $\frac{3\cdot5\cdot21}{4\cdot12\cdot32}ac^4$, in the second theorem, and for those which correspond to $\frac{1}{2}acc$, and $\frac{9}{2\cdot16}ac^4$, in the third theorem, is saved. These are the expedients which I am next to explain.

7. The three terms a, $\frac{3}{8}acc$, and $\frac{3\cdot5}{8\cdot8}ac^4$, which are found in the first theorem, are evidently to the three terms $\frac{3}{4}a$, $\frac{3\cdot5}{4\cdot12}acc$, and $\frac{3\cdot5\cdot21}{4\cdot12\cdot32}ac^4$, which are found in the second theorem, in the ratio of 1 to $\frac{3}{4}$, 1 to $\frac{5}{6}$, and 1 to $\frac{7}{8}$ respectively; or as 1 to $1-\frac{1}{4}$, 1 to $1-\frac{1}{6}$, and 1 to $1-\frac{1}{8}$; by which mixed numbers, the logarithmic terms in the second theorem may more easily be derived from those in the first theorem, than by the fractions, as will appear further on.

8. It is no less evident, that the three logarithmic terms a, $\frac{1}{6}acc$, and $\frac{3\cdot5}{8.8}ac^4$, mentioned in the preceding Art. are to the three logarithmic terms 2a, $\frac{1}{2}acc$, and $\frac{9}{2.16}ac^4$, which occur in the third theorem, in the ratio of 1 to 2, 1 to $\frac{4}{3}$, and 1 to $\frac{6}{5}$ respectively; or as 1 to $1+1$, 1 to $1+\frac{1}{3}$, and 1 to $1+\frac{1}{5}$; by which mixed numbers, as was observed in the preceding Art. the logarithmic terms in the third theorem may be more easily derived from those in the first theorem, than by the fractions.

9. The first of the logarithmic terms in the first theorem has already been denoted by the Roman letter a; now let the second and third, *viz.* $\frac{1}{6}acc$, and $\frac{3\cdot5}{8.8}ac^4$, be denoted by the Roman letters b and c respectively; and let the sum of these three terms, *viz.* $a+\frac{1}{6}acc+\frac{3\cdot5}{8.8}ac^4$, now denoted by $a+b+c$, be put $=S$; then, by Art. 7. the logarithmic terms in the second theorem will be $(1-\frac{1}{4})a$, $(1-\frac{1}{6})b$, and $(1-\frac{1}{8})c$; and the sum of these terms will be $a+b+c-\frac{a}{4}-\frac{b}{6}-\frac{c}{8}=S-\frac{a}{4}-\frac{b}{6}-\frac{c}{8}$, where S is given, it being $=$ the three logarithmic terms in the first theorem, with which the computation ought to begin; and the $\frac{1}{4}$, $\frac{1}{6}$, and $\frac{1}{8}$ of these terms respectively, are very easily computed without the use of logarithms, as will hereafter appear by an example.

And the logarithmic terms in the third theorem will likewise be denoted by 2a, $(1+\frac{1}{3})b$, and $(1+\frac{1}{5})c$ respectively; the sum of which is $=a+b+c+a+\frac{1}{3}b+\frac{1}{5}c=S+a+\frac{b}{3}+\frac{c}{5}$, where S, as well as a, b, and c, being given, the fractional parts are very easily computed without the use of logarithms.

10. Having now described these short and easy methods of

computing the values of a, b, and c, and of deriving the other logarithmic terms from them, and having introduced a new and more compendious notation of several of the terms in each of the first three theorems, it will be proper next to exhibit those theorems in this improved state, and, after that, to give an example or two of computing by them.

1. $A = \frac{1}{\pi(a+b)^{\frac{1}{2}}} \times \left\{ \begin{array}{l} \frac{2}{cc} + e - fcc - gc^4 \\ +a + \frac{3}{8}acc\,(=b) + \frac{3\cdot5}{8\cdot8}ac^4\,(=c). \end{array} \right.$

2. $A' = \frac{1}{\pi(a+b)^{\frac{1}{2}}} \times \left\{ \begin{array}{l} \frac{4}{3c^3} + \frac{1}{cc} + b - icc - kc^4 \\ +S - \frac{a}{4} - \frac{b}{6} - \frac{c}{8}. \end{array} \right.$

3. $B = \frac{2a}{b}A - \frac{2}{\pi b(a+b)^{\frac{1}{2}}} \times \left\{ \begin{array}{l} \rho - lcc - mc^4 \\ +S + a + \frac{b}{3} + \frac{c}{5}. \end{array} \right.$

Or, putting $\underset{I}{A}$ to denote the product of $\frac{1}{\pi(a+b)^{\frac{1}{2}}} \times \left\{ \begin{array}{l} \rho - lcc - mc^4 \\ +S + a + \frac{b}{3} + \frac{c}{5}, \end{array} \right.$

this theorem will be more concisely and commodiously expressed thus; $B = \frac{2}{b}(Aa - \underset{I}{A})$.

4. $B' = \frac{2}{b}(A'a - A)$. N. B. $S = a + b + c$.

11. We might now proceed to an example of computing by these theorems; but it will be very convenient first to set down the constant numbers and constant logarithms which are to be used in these computations.

The constant numbers, taken from Art. 12 of the first Appendix, and Art. 2, 3, and 4 of this Appendix, are the following:

$e = 0{\cdot}1931{,}472,$	$b = 0{\cdot}0823{,}604,$	$\rho = 1{\cdot}3862{,}944,$
$f = 0{\cdot}1463{,}198,$	$i = 0{\cdot}1323{,}498,$	$l = 0{\cdot}1534{,}264,$
$g = 0{\cdot}1187{,}936,$	$k = 0{\cdot}1076{,}091,$	$m = 0{\cdot}1381{,}774.$

And the constant logarithms to be used in these calculations are the following, which are respectively set down to as many places of figures as are requisite.

L. 2 = 0·3010,300, L. $\frac{1}{8}$ = $\overline{1}$·5740,3, L. $\frac{5}{8}$ = $\overline{1}$·796,
L. $\frac{2}{3}$ = $\overline{1}$·8239,087, L. $\frac{4}{3}$ = 0·1249,4, L. π = 0·4971,499;
L. f = $\overline{1}$·1653,0, L. i = $\overline{1}$·1217,2, L. l = $\overline{1}$·1859,1,
L. $\frac{g}{f}$ = $\overline{1}$·910, L. $\frac{k}{i}$ = $\overline{1}$·910, L. $\frac{m}{l}$ = $\overline{1}$·939.

By comparing this Art. with Art. 13 of the first Appendix, it will appear, that the number of logarithms used in the new *formulæ* is very considerably less than the number used in those from which they were derived; and still fewer will suffice, since the term $\frac{4}{3c^4}$, which occurs in the second theorem, is most easily derived from $\frac{2}{cc}$,* the first term in the first theorem, the logarithms of $\frac{2}{cc}$, $\frac{2}{3}$, and cc, being there ready calculated; so that L. $\frac{4}{3}$ needs not be used in the computation.

12. Let us now compute A and B by the first and third of these new *formulæ*, when Venus and the earth are the two planets.

* See Art. 11 of the first Appendix.

First, for the value of A.

	Numbers.	Logarithms.	Numbers.*
Here $a = 1{\cdot}5236{,}71$ and $b = 1{\cdot}4451{,}60$	Ar. co.	$0{\cdot}1828{,}913$ $\overline{1}{\cdot}8400{,}841$	
$a - b = 0{\cdot}0785{,}11$		$\overline{2}{\cdot}8949{,}305$	
$a + b = 2{\cdot}9688{,}31$		$0{\cdot}4725{,}855$	
$\frac{a-b}{a+b} = cc$	- -	$\overline{2}{\cdot}4223{,}450$	
$\frac{2}{cc}$	-	$1{\cdot}8786{,}850$	$75{\cdot}62841 = \frac{2}{cc}$
			$0{\cdot}19315 = e$
			$75{\cdot}82156 = \frac{2}{cc} + e$
fcc	-	$\overline{3}{\cdot}5876{,}5$	$-0{\cdot}00387 = -fcc$
$\frac{g}{f}cc$	-	$\overline{2}{\cdot}332$	
Sum of these two logarithms $\overline{5}{\cdot}920$	-		$-0{\cdot}00008 = -gc^4$
			$-0{\cdot}00395 = -fcc - gc^4$
The sum of these four terms is	- -		$75{\cdot}81761$.

Having now found the value of the four terms $\frac{2}{cc} + e - fcc - gc^4$, we must next find the value of the three logarithmic terms $a + \frac{3}{8}acc + \frac{3\cdot5}{8\cdot8}ac^4$, or $a + b + c = S$, which may quickly and easily be done as follows.

The common logarithm of 2 is $0{\cdot}3010{,}300$

Half the common logarithm of cc is $\overline{1}{\cdot}2111{,}725$

The common logarithm of $\frac{2}{c}$ is $1{\cdot}0898{,}575$; and this logarithm, reduced to an hyperbolic logarithm, by Table XXXVII.

* See Art. 14 of the first Appendix, paragraph the third.

of Dodson's Calculator, gives - - $2\cdot50949 = a.$

a	$0\cdot39959$	
$\frac{1}{6}cc$	$\bar{3}\cdot99638$	
Sum of these two logarithms	$2\cdot39597$	$0\cdot02489 = \frac{1}{6}acc = b$
$\frac{1}{8}cc$	$2\cdot218$	
Sum of these two logarithms	$4\cdot614$	$0\cdot000041 = \frac{3\cdot5}{8\cdot8}ac^4 = c$

The sum of these three terms is - $2\cdot53479 = S$; to which add the sum of the four terms above found $75\cdot81761$, and we have

$78\cdot35240 =$ all the terms.

	$1\cdot8940,523$	
$\pi(a+b)^{\frac{1}{2}}$	$1\cdot2060,282$	
The diff. of these two logarithms is - -	$0\cdot6880,241$	$4\cdot87555 = A.$

13. We are next to compute the value of B; which computation will be much facilitated by the use of numbers already found in the computation of the value of A. The arithmetical work may stand as follows.

		$1.38630 = p$
lcc	$\bar{3}.60826$	$-0.00406 = -lcc$
$\frac{m}{l}cc$	$\bar{2}.361$	
	$\bar{5}.969$	$-0.00009 = -mc^4$
		$1.38215 =$ the sum of these three terms.
		$2.53479 = S$
		$2.50949 = a$
		$0.00830 = \frac{b}{3}$
		$0.00008 = \frac{c}{5}$
	$0.8085,357$	$6.43481 =$ the sum of all the terms.
$\pi\,(a+b)^{\frac{1}{2}}$	$0.7334,427$	
Diff. of these two log^s.	$0.0750,980$	$1.18874 = A$
A	$0.6880,241$	
a	$0.1828,913$	
Sum of these two log^s.	$0.8709,154$	$7.42874 = Aa$
	$0.7951,846$	$6.24000 = Aa - A$
$\frac{2}{b}$	$0.1411,141$	
$\frac{2}{b}(Aa - A)$	$0.9362,987$	$8.63572 = B.$

14. We have now the values sought, *viz.* $A = 4.8756$, and $B = 8.6357$; which values, computed by the new *formulæ*, agree with those which were given in the first Appendix, which is one proof of the accuracy of the new forms to which the theorems are brought. And, that the calculations of these numbers are considerably facilitated and abridged by the use of H. L. $\frac{2}{c} - \frac{cc}{4} - \frac{3c^4}{4.8}$ instead of H. L. $\frac{1+\sqrt{(1-cc)}}{c}$, and by the

easy method of deriving the logarithmic terms in the second and third theorems from those in the first, will quickly appear to any one who shall make trial, by a calculation both by the original *formulæ* and by those which are given in this paper, or who shall compare the computations of A and B in the first Appendix, with the computations here exhibited.

VI. *Account of a Peculiarity in the Distribution of the Arteries sent to the Limbs of slow-moving Animals; together with some other similar Facts. In a Letter from Mr.* Anthony Carlisle, *Surgeon, to* John Symmons, *Esq. F. R. S.*

Read Jan. 9, 1800.

DEAR SIR,

THE Maucauco you have been so obliging as to give me for the purpose of dissection, has proved a subject of considerable interest. This animal, the *Lemur tardigradus* of LINNÆUS, was injected, with a view to exhibit the course of the arteries; and they present a very unusual deviation from the ordinary arrangement of this class of blood-vessels in animals generally. Before I had leisure to inquire further into this peculiarity, I presented a drawing of the appearances to my friend Dr. SHAW, of the British Museum, for the purpose of being made public in his work of natural history, now in the press. Since that time, I have, through Dr. SHAW's assistance, been enabled to investigate this subject somewhat farther; and, if you consider the following account in any degree worthy the attention of the Royal Society, I shall receive an additional honour by its proceeding through your hands.

The *Lemur tardigradus*, in its injected state, accompanies this paper; and, for the kind of preparation, the vessels are filled with more than ordinary success. The arteries alone are injected; and the peculiarity of their arrangement is to be observed in the axillary arteries, and in the iliacs. These vessels, at their entrance into the upper and lower limbs, are suddenly divided

into a number of equal-sized cylinders, which occasionally anastomose with each other. They are exclusively distributed on the muscles; whilst the arteries sent to all the parts of the body, excepting the limbs, divide in the usual arborescent form; and, even those arteries of the limbs which are employed upon substances not muscular, branch off like the common blood-vessels. I counted twenty-three of these cylinders, parallel to each other, about the middle of the upper arm; and seventeen in the inguinal fasciculus.*

This fact appeared at first too solitary for the foundation of any physiological reasoning; but, having since had an opportunity of prosecuting the inquiry, among animals of similar habits and character, I have been encouraged to hope that the result may eventually assist in the elucidation of muscular motion. The *Bradypus tridactylus*, or great American Sloth, has a similar distribution of the arteries of its limbs to that already described in the *Lemur tardigradus*; which will be better understood by the annexed figures. See Plate II. figs. 1 and 2; and the explanations. The communications of these vessels with each other are more frequent than in the *Lemur tardigradus*, and their number is considerably greater. I counted forty-two separate cylinders upon the superficies of the brachial fasciculus; and, from the bulk of the fasciculus, I estimate that there were twenty, or more, concealed in the middle. The lower extremity has its arteries less divided, and they are of larger diameter. I observed only thirty-four branches in the middle of the thigh; and the first series of ramifications were larger than the subsequent ones. May not this have some relation to the greater distance of the lower limb from the

* See Plate I. and its references, page 103.

heart? The extremely slow movements of the *Bradypus tridactylus* are sufficiently known among natural historians.

The *Bradypus didactylus* has its arterial system distributed in some degree like the *tridactylus*; but the brachial artery in the upper limb is much less subdivided, as will appear by the representation in Plate II. fig. 3; and, in the lower limb, the arteries of the plexus afterwards divide a few times in the arborescent form. It may be worthy of remark, that this correspondence of arrangement, in the arteries of the lesser Sloth, bears a striking analogy with the structure and habits of the large American Sloth; the movements of the *Bradypus didactylus* being universally represented quicker than those of the *Bradypus tridactylus*.

The *Lemur Loris* was next examined, and its arterial system was found to resemble those already described; but, as the animal had been preserved in very strong spirit, the vessels were so corrugated as not to admit of injection. The two *Bradypi* were injected with quicksilver. The natural history of the *Lemur Loris* appears not to be very well ascertained; but it is a slow-moving animal, and has been confounded with the species called *tardigradus*, although doubtless a much more agile creature. See Plate II. figs. 5, and 6.

In all the quadrupeds before mentioned, the other blood-vessels, as well as the nerves, presented the common appearances. The size of the heads, and the interior capacity of the skulls, both in the *Bradypus tridactylus* and the *Lemur tardigradus*, seemed smaller in proportion than is usual among animals, so that the quantity of brain must be less than ordinary.

The effect of this peculiar disposition of the arteries, in the limbs of these slow-moving quadrupeds, will be that of retarding the velocity of the blood. It is well known, and has been

explained by various writers, that the blood moves quicker in the arteries near the heart, than in the remote branches; and also, that fluids move more rapidly through tubes which branch off suddenly from large trunks, than if they had been propelled for a considerable distance through small-sized cylinders; besides which, the frequent communications in the cylinders of the *Bradypus tridactylus* must produce eddies, which will retard the progress of the fluid. From these and a variety of other facts, which it is not necessary to specify, it will appear, that one effect upon the animal economy, connected with this arrangement of vessels, must be, that of diminishing the velocity of the blood passing into the muscles of the limbs. It may be difficult to determine, whether the slow movement of the blood sent to these muscles be a subordinate convenience to other primary causes of their slow contraction, or whether it be of itself the immediate and principal cause. The facts at present ascertained, relative to muscular motion, do not authorize me to treat decidedly of the share which the vascular system holds in the operation of muscular contraction. Certain it is, that a larger proportion of arteries is sent to the muscles of quadrupeds, than to the ordinary substances; and the extreme redness of these organs shews that their capillaries are of large diameter. A greater degree of redness is also observable in those muscles (of the same animal) which are most frequently called into action. The habits of life among the tardigrade animals, give occasion for the long continued contraction of some muscles in their limbs: these creatures are represented clinging to the boughs of trees, and remaining thus, without locomotion, for several hours. The powers which require so long a time to determine the contraction of a series of muscles, are probably

no less slow in restoring the parts to their former condition; or, if the restoration is to be effected by antagonist muscles under the same circumstances, then, the flexion and extension of every part of the limbs will correspond, as to time.

I have not met with any arrangement of blood-vessels analogous to those described, except in the carotid artery of the Lion. May not this peculiarity be subservient to the long continued exertion of the muscles of his jaws, whilst holding a powerful animal, such as a Horse or Buffalo, and thus enable him to retain his prey, until it is wearied out by ineffectual struggles? I believe also, that those animals which chew the cud, have a plexus of arteries in the neck, analogous to the *rete mirabile:* but this fact has not yet been verified in all the ruminating quadrupeds; and the effect of these arrangements seems rather to operate as sluices to the arteries of the masticating muscles, than directly as the means of retarding the velocity of their fluids. It is however necessary to examine these subjects more accurately.*

As I have instituted a series of experiments and inquiries, with the hope of elucidating this subject, it would be improper to trouble you, or the Royal Society, with any physiological reasonings until these are completed.

I have the honour to be, &c.

ANT. CARLISLE.

Soho Square,
October 28th, 1799.

* There is a *rete mirabile* in the genus *Bos*, and in some of the *Cervi* which I have seen; but of these and the other *Pecora* a fuller description will be given in a future paper.

P. S. The Maucauco which you lately possessed, was sufficiently quick in the movements of its head to snap a person's finger, when touched incautiously; and the motion of its jaw, when chewing, was not slower than in other animals. A Maucauco of the same species, kept among the wild beasts in the Tower of London, was very apt to bite those who, calculating the movements of its head by those of its limbs, approached within the length of its neck: the chewing of this animal was similar to that of a Cat. These external habits of motion, compared with those of the limbs, coincide very much with the internal structure here described.

REFERENCES TO THE FIGURES.

Plate I.

The figure represents a dried preparation of the *Lemur tardigradus*, exhibiting the appearances of the arterial system.

a, the carotid arteries.

b, the axillary artery, dividing into the plexus described.

c, the iliac arteries, dividing into the cylindrical ramifications.

The other parts of the arterial system are represented according to the natural and ordinary disposition.

Plate II.

Fig. 1, shews the axilla of the *Bradypus tridactylus*, dissected to expose the vessels.

a, the sterno mastoideus muscle, passing under the skin of the neck.

b, part of the axillary plexus of nerves; the median proceeding along the arm, with the large blood-vessels, and giving off two branches of communication with the ulnar nerve.

c, the subclavian vein.

d, the first bone of the sternum, attached to its second bone, and to the first and second ribs.

e, the subclavian artery, passing into the axilla behind the large veins, and dividing itself into a great number of equal-sized cylinders, which cling together, frequently anastomose, and take the ordinary route of the main trunks of arteries in the upper limbs of quadrupeds.

Fig. 2, the brim of the pelvis and groin of the *Bradypus tridactylus*, with the vessels exposed.

a, the aorta, where it divides into the two great iliac branches; the iliac artery on the right side being continued, to shew the division into the anastomosing cylinders which are sent to the muscles.

b, part of the iliacus internus muscle.

c, part of the bony margin of the pelvis leading down to the pubes.

Fig. 3, the upper limb of the *Bradypus didactylus*.

a, a portion of skin on the top of the shoulder.

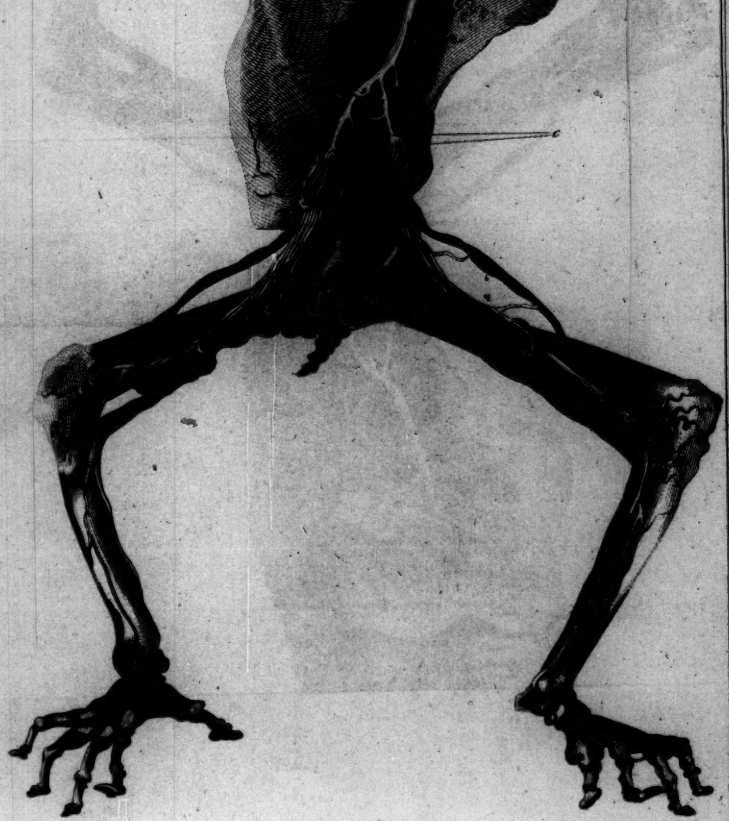
b, the axillary artery, divided more in the ordinary way than in the former animal. This creature had been preserved in ardent spirit containing camphor; and the dissection could not be prosecuted so satisfactorily as to expose every small branch.

c, part of the axillary plexus of nerves.

Fig. 4, the iliac vessels of the *Bradypus didactylus*.

a, the tendon of the psoas muscle.

b, the iliac artery, proceeding into the thigh, where its divisions are more discernible than in the upper limb. The cylindric tubes were however much fewer than in the *Bradypus tridactylus*. I counted only eight tubes in the inguinal fasciculus



a
b
c
d
e

Fig. 1.

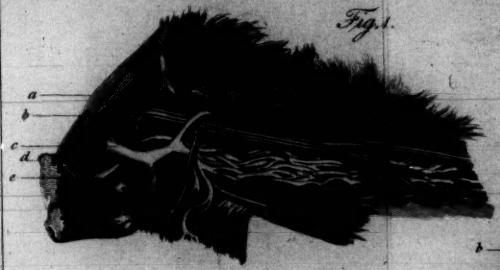


Fig. 2.

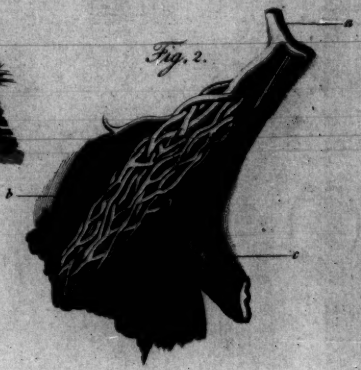


Fig. 3.

Fig. 4.

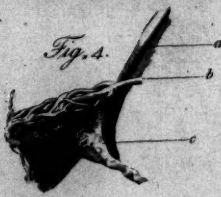


Fig. 5.

Fig. 6.

J. C. del.

F. Bagster sc.

of this Sloth, whereas in the same part of the *tridactylus* there were at least forty.

c, part of the rim of the pelvis.

Fig. 5, the upper limb of the *Lemur Loris*.

a, the head of the os brachii.

b, the axillary artery, proceeding along the arm, and dividing into seven or eight cylinders.

Fig. 6, the inguinal arteries of the *Lemur Loris*.

a, the iliac artery, dividing as it passes the groin into five or six cylinders.

b, the bony margin of the pelvis.

The figures are of the size of the different natural objects.

VII. *Outlines of Experiments and Inquiries respecting Sound and Light.* By Thomas Young, *M. D. F. R. S. In a Letter to* Edward Whitaker Gray, *M. D. Sec. R. S.*

Read January 16, 1800.

DEAR SIR,

It has long been my intention to lay before the Royal Society a few observations on the subject of sound; and I have endeavoured to collect as much information, and to make as many experiments, connected with this inquiry, as circumstances enabled me to do; but, the further I have proceeded, the more widely the prospect of what lay before me has been extended; and, as I find that the investigation, in all its magnitude, will occupy the leisure hours of some years, or perhaps of a life, I am determined, in the mean time, lest any unforeseen circumstances should prevent my continuing the pursuit, to submit to the Society some conclusions which I have already formed from the results of various experiments. Their subjects are, I. The measurement of the quantity of air discharged through an aperture. II. The determination of the direction and velocity of a stream of air proceeding from an orifice. III. Ocular evidence of the nature of sound. IV. The velocity of sound. V. Sonorous cavities. VI. The degree of divergence of sound. VII. The decay of sound. VIII. The harmonic sounds of pipes. IX. The vibrations of different elastic fluids. X. The analogy between light and sound. XI. The coalescence of musical sounds. XII.

The frequency of vibrations constituting a given note. XIII. The vibrations of chords. XIV. The vibrations of rods and plates. XV. The human voice. XVI. The temperament of musical intervals.

I. *Of the Quantity of Air discharged through an Aperture.*

A piece of bladder was tied over the end of the tube of a large glass funnel, and punctured with a hot needle. The funnel was inverted in a vessel of water; and a gage, with a graduated glass tube, was so placed as to measure the pressure occasioned by the different levels of the surfaces of the water. As the air escaped through the puncture, it was supplied by a phial of known dimensions, at equal intervals of time; and, according to the frequency of this supply, the average height of the gage was such as is expressed in the first Table. It appears, that the quantity of air discharged by a given aperture, was nearly in the subduplicate ratio of the pressure; and that the ratio of the expenditures by different apertures, with the same pressure, lay between the ratio of their diameters and that of their areas. The second, third, and fourth Tables show the result of similar experiments, made with some variations in the apparatus. It may be inferred, from comparing the experiments on a tube with those on a simple perforation, that the expenditure is increased, as in water, by the application of a short pipe.

Table I.

A	B	C
.00018	.25	3.9
.00018	.58	11.7
.00018	1.	15.6
.001	.045	7.8
.001	.2	15.6
.001	.7	31.2
.004	.35	46.8

A is the area, in square inches, of an aperture nearly circular. B, the pressure in inches. C, the number of cubic inches discharged in one minute.

All numbers throughout this paper, where the contrary is not expressed, are to be understood of inches, linear, square, or cubic.

Table II.

A	B	C
.07	1.	2000.
.07	2.	2900.

A is the area of the section of a tube about two inches long. B, the pressure. C, the quantity of air discharged in a minute, by estimation.

Table III.

A	B	C	D
.0064	1.15	.2	46.8
.0064	10.	.45	46.8
.0064	13.5	.35	31.2
.0064	13.5	.7	46.8

A is the area of the section of a tube. B, its length. C, the pressure. D, the discharge in a minute.

Table IV.

A	B	C
.003	.28.	46.8

A is the area of an oval aperture, formed by flattening a glass tube at the end: its diameters were .025 and .152. B, the pressure. C, the discharge.

II. *Of the Direction and Velocity of a Stream of Air.*

An apparatus was contrived for measuring, by means of a water-gage communicating with a reservoir of air, the pressure by which a current was forced from the reservoir through a cylindrical tube; and the gage was so sensible, that, a regular blast being supplied from the lungs, it showed the slight variation produced by every pulsation of the heart. The current of air issuing from the tube was directed downwards, upon a white plate, on which a scale of equal parts was engraved, and which was thinly covered with a coloured liquid; the breadth of the surface of the plate laid bare was observed at different distances from the tube, and with different degrees of pressure, care being taken that the liquid should be so shallow as to yield to the slightest impression of air. The results are collected in Tables v. and vi. and are exhibited to the eye in Plate III. Figs. 1—12. In order to measure with greater certainty and precision, the velocity of every part of the current, a second cavity, furnished with a gage, was provided, and pieces perforated with apertures of different sizes were adapted to its orifice: the axis of the current was directed as accurately as possible to the centres of these apertures, and the result of the experiments, with various pressures and distances, are inserted in Tables vii. viii. and ix. The velocity of a stream being, both according to the commonly received opinion and to the experiments already related, nearly in the subduplicate ratio of the pressure occasioning it, it was inferred, that an equal pressure would be required to stop its progress, and that the velocity of the current, where it struck against the aperture, must be in the subduplicate ratio of the pressure marked by the gage. The ordi-

nates of the curves in Figs. 19—23, were therefore taken reciprocally in the subduplicate ratio of the pressure marked by the second gage to that indicated by the first, at the various distances represented by the abscisses. Each figure represents a different degree of pressure in the first cavity. The curve nearest the axis, is deduced from observations in which the aperture opposed to the tube was not greater than that of the tube itself; and shows what would be the diameter of the current, if the velocities of every one of its particles in the same circular section, including those of the contiguous air, which must have acquired as much motion as the current has lost, were equal among themselves. As the central particles must be supposed to be less impeded in their motion than the superficial ones, of course, the smaller the aperture opposed to the centre of the current, the greater the velocity ought to come out, and the ordinate of the curve the smaller; but, where the aperture was not greater than that of the tube, the difference of the velocities at the same distance was scarcely perceptible. When the aperture was larger than that of the tube, if the distance was very small, of course, the average velocity came out much smaller than that which was inferred from a smaller aperture; but, where the ordinate of the internal curve became nearly equal to this aperture, there was but little difference between the velocities indicated with different apertures. Indeed, in some cases, a larger aperture seemed to indicate a greater velocity: this might have arisen in some degree from the smaller aperture not having been exactly in the centre of the current; but there is greater reason to suppose, that it was occasioned by some resistance derived from the air returning between the sides of the aperture and the current entering it. Where

this took place, the external curves, which are so constructed as that their ordinates are reciprocally in the subduplicate ratio of the pressure observed in the second cavity, with apertures equal in semidiameter to their initial ordinate, approach, for a short distance, nearer to the axis than the internal curve: after this, they continue their course very near to this curve. Hence it appears, that no observable part of the motion diverged beyond the limits of the solid which would be formed by the revolution of the internal curve, which is seldom inclined to the axis in an angle so great as ten degrees. A similar conclusion may be made, from observing the flame of a candle subjected to the action of a blowpipe: there is no divergency beyond the narrow limits of the current; the flame, on the contrary, is every where forced by the ambient air towards the current, to supply the place of that which it has carried away by its friction. The lateral communication of motion, very ingeniously and accurately observed in water by Professor VENTURI, is exactly similar to the motion here shown to take place in air; and these experiments fully justify him in rejecting the tenacity of water as its cause: no doubt it arises from the relative situation of the particles of the fluid, in the line of the current, to that of the particles in the contiguous strata, which is such as naturally to lead to a communication of motion nearly in a parallel direction; and this may properly be termed friction. The lateral pressure which urges the flame of a candle towards the stream of air from a blowpipe, is probably exactly similar to that pressure which causes the inflection of a current of air near an obstacle. Mark the dimple which a slender stream of air makes on the surface of water; bring a convex body into contact with the side of the stream, and the place of the dimple

will immediately show that the current is inflected towards the body; and, if the body be at liberty to move in every direction, it will be urged towards the current, in the same manner as, in VENTURI'S experiments, a fluid was forced up a tube inserted into the side of a pipe through which water was flowing. A similar interposition of an obstacle in the course of the wind, is probably often the cause of smoky chimneys. One circumstance was observed in these experiments, which it is extremely difficult to explain, and which yet leads to very important consequences: it may be made distinctly perceptible to the eye, by forcing a current of smoke very gently through a fine tube. When the velocity is as small as possible, the stream proceeds for many inches without any observable dilatation; it then immediately diverges at a considerable angle into a cone, Plate IV. Fig. 24; and, at the point of divergency, there is an audible and even visible vibration. The blowpipe also affords a method of observing this phænomenon: as far as can be judged from the motion of the flame, the current seems to make something like a revolution in the surface of the cone, but this motion is too rapid to be distinctly discerned. When the pressure is increased, the apex of the cone approaches nearer to the orifice of the tube, Figs. 25, 26; but no degree of pressure seems materially to alter its divergency. The distance of the apex from the orifice, is not proportional to the diameter of the current; it rather appears to be the greater the smaller the current, and is much better defined in a small current than in a large one. Its distance in one experiment is expressed in Table x, from observations on the surface of a liquid; in other experiments, its respective distances were sometimes considerably less with the same degrees of pressure. It may be inferred, from the numbers of Tables VII and VIII,

that in several instances a greater height of the first gage produced a less height of the second: this arose from the nearer approach of the apex of the cone to the orifice of the tube, the stream losing a greater portion of its velocity by this divergence than it gained by the increase of pressure. At first sight, the form of the current bears some resemblance to the *vena contracta* of a jet of water: but VENTURI has observed, that in water an increase of pressure increases, instead of diminishing, the distance of the contracted section from the orifice. Is it not possible, that the facility with which some spiders are said to project their fine threads to a great distance, may depend upon the small degree of velocity with which they are thrown out, so that, like a minute current, meeting with little interruption from the neighbouring air, they easily continue their course for a considerable time?

Table v.

A	1.	2.	3.	3.8
B	C	C	C	C
1.	.1	.1	.1	
2.	.12	.12	.2	
3.	.17	.25	.3	
4.	.2	.4	.4	
5.	.25	.5		
6.	.30	.52		
7.	.35	.54	.5	
8.	.37	.56		
9.	.39	.58		
10.	.40	.6	.6	.5
15.		.7		
18.	.50			
20.				

The diameter of the tube .07. A is the distance of the liquid from the orifice. B, the pressure. C, the diameter of the surface of the liquid displaced.

Table VI.

A	1.	2.
B	C	C
1.	.1	.1
2.	.13	
3.	.2	.2
4.	.25	.3
6.	.3	.4
7.	.35	.5
10.	.35	.6
15.	.35	.7
20.	.35	.7

Diameter of the tube, .1. A, B, and C, as in Table v.

Table VII.

A	.5	
B	.06	.15
C	D	D
.1	.083	
.2	.16	
.3	.25	.1
.4	.35	
.5	.45	
.6	.53	.2
.7	.6	
.8		.3
1.	.5	
1.2	.4	.4
1.5	.6	
2.	.67	.55
4.	1.3	1.
8.		2.
9.	.3	
14.	.5	

Diameter of the tube .06.

A is the distance of the opposite aperture, from the orifice of the tube. B, the diameter of the aperture. C, the pressure, indicated by the first gage. D, the height of the second gage.

Table VIII.

A	.5				1.				2.				4.			
B	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	5.	.06	.15	.3	.5
C	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D
.1	.05	.05			.03				.017							
.2	.1	.1			.12	.08	.02		.034							
.5	.2	.22			.1	.00			.00							
1.	.32	.36	.1		.17	.1	.1	.05	.04							
2.	.52	.6	.2		.28	.22	.21	.08	.07							
3.	.8	.9	.3		.4	.36	.32	.12	.12		.1	.1				
4.	1.1	1.2	.4		.58	.52	.42	.16	.18		.15	.14				
5.		1.5	.5		.8	.68	.52	.2	.23		.2	.18	.04		.04	.05
6.		1.7	.6		1.	.83	.63	.25	.3		.25	.22	.05		.05	.06
7.		1.9	.7		1.2	1.	.75	.3	.35		.3	.26	.06		.06	.07
8.		2.1	.8		1.5	1.2	.88	.34	.4		.34	.3	.07		.07	.07
9.		2.3	.9		1.7	1.4	1.	.37	.45		.37	.34	.08		.08	.08
10		2.6	1.		1.9	1.6	1.1	.4	.5		.4	.37	.09		.09	.09

Diameter of the tube .1. A, B, C, and D, as in Table VII.

Table IX.

A	1.15				3.3			4.
B	.15	.3	.5	1.	.06	.15	1.	.06
C	D	D	D	D	D	D	D	D
.5	.1	.1	.1					
1.	.2	.2	.2					
2.	.4	.35	.34	.13	.1	.1	.125	
3.	.6	.5	.5	.2	.15	.15	.18	.1

Diameter of the tube .3.

A, B, C, and D, as in Table VII.

Table X.

A	B
.4	6.
.8	3.
1.2	1.5
1.8	1.
2.	.5
4.	.0

A is the pressure, B, the distance of the apex of the cone from the orifice of a tube .1 in diameter.

III. *Ocular Evidence of the Nature of Sound.*

A tube about the tenth of an inch in diameter, with a lateral orifice half an inch from its end, filed rather deeper than the axis of the tube, Fig. 27, was inserted at the apex of a conical cavity containing about twenty cubic inches of air, and luted perfectly tight: by blowing through the tube, a sound nearly in unison with the tenor C was produced. By gradually increasing the capacity of the cavity as far as several gallons, with the same mouth-piece, the sound, although faint, became more and more grave, till it was no longer a musical note. Even before this period a kind of trembling was distinguishable; and this, as the cavity was still further increased, was changed into a succession of distinct puffs, like the sound produced by an explosion of air from the lips; as slow, in some instances, as 4 or 3 in a second. These were undoubtedly the single vibrations, which, when repeated with sufficient frequency, impress on the auditory nerve the sensation of a continued sound. On forcing a current of smoke through the tube, the vibratory motion of the stream, as it passed out at the lateral orifice, was evident to the eye; although, from various circumstances, the quantity and direction of its motion could not be subjected to

exact mensuration. This species of sonorous cavity seems susceptible of but few harmonic sounds. It was observed, that a faint blast produced a much greater frequency of vibrations than that which was appropriate to the cavity: a circumstance similar to this obtains also in large organ pipes; but, several minute observations of this kind, although they might assist in forming a theory of the origin of vibrations, or in confirming such a theory drawn from other sources, yet, as they are not alone sufficient to afford any general conclusions, are omitted at present, for the sake of brevity.

IV. *Of the Velocity of Sound.*

It has been demonstrated, by M. DE LA GRANGE and others, that any impression whatever communicated to one particle of an elastic fluid, will be transmitted through that fluid with an uniform velocity, depending on the constitution of the fluid, without reference to any supposed laws of the continuation of that impression. Their theorem for ascertaining this velocity is the same as NEWTON has deduced from the hypothesis of a particular law of continuation: but it must be confessed, that the result differs somewhat too widely from experiment, to give us full confidence in the perfection of the theory. Corrected by the experiments of various observers, the velocity of any impression transmitted by the common air, may, at an average, be reckoned 1130 feet in a second.

V. *Of sonorous Cavities.*

M. DE LA GRANGE has also demonstrated, that all impressions are reflected by an obstacle terminating an elastic fluid, with the same velocity with which they arrived at that obstacle.

When the walls of a passage, or of an unfurnished room, are smooth and perfectly parallel, any explosion, or a stamping with the foot, communicates an impression to the air, which is reflected from one wall to the other, and from the second again towards the ear, nearly in the same direction with the primitive impulse: this takes place as frequently in a second, as double the breadth of the passage is contained in 1130 feet; and the ear receives a perception of a musical sound, thus determined in its pitch by the breadth of the passage. On making the experiment, the result will be found accurately to agree with this explanation. If the sound is predetermined, and the frequency of vibrations such as that each pulse, when doubly reflected, may coincide with the subsequent pulse proceeding directly from the sounding body, the intensity of the sound will be much increased by the reflection; and also, in a less degree, if the reflected pulse coincides with the next but one, the next but two, or more, of the direct pulses. The appropriate notes of a room may readily be discovered by singing the scale in it; and they will be found to depend on the proportion of its length or breadth to 1130 feet. The sound of the stopped diapason pipes of an organ is produced in a manner somewhat similar to the note from an explosion in a passage; and that of its reed pipes to the resonance of the voice in a room: the length of the pipe in one case determining the sound, in the other, increasing its strength. The frequency of the vibrations does not at all immediately depend on the diameter of the pipe. It must be confessed, that much remains to be done in explaining the precise manner in which the vibration of the air in an organ pipe is generated. M. DANIEL BERNOULLI has solved several difficult

problems relating to the subject; yet some of his assumptions are not only gratuitous, but contrary to matter of fact.

VI. *Of the Divergence of Sound.*

It has been generally asserted, chiefly on the authority of NEWTON, that if any sound be admitted through an aperture into a chamber, it will diverge from that aperture equally in all directions. The chief arguments in favour of this opinion are deduced from considering the phænomena of the pressure of fluids, and the motion of waves excited in a pool of water. But the inference seems to be too hastily drawn: there is a very material difference between impulse and pressure; and, in the case of waves of water, the moving force at each point is the power of gravity, which, acting primarily in a perpendicular direction, is only secondarily converted into a horizontal force, in the direction of the progress of the waves, being at each step disposed to spread equally in every direction: but the impulse transmitted by an elastic fluid, acts primarily in the direction of its progress. It is well known, that if a person calls to another with a speaking trumpet, he points it towards the place where his hearer stands: and I am assured by a very respectable Member of the Royal Society, that the report of a cannon appears many times louder to a person towards whom it is fired, than to one placed in a contrary direction. It must have occurred to every one's observation, that a sound such as that of a mill, or a fall of water, has appeared much louder after turning a corner, when the house or other obstacle no longer intervened; and it has been already remarked by EULER, on this head, that we are not acquainted with any substance perfectly

impervious to sound. Indeed, as M. LAMBERT has very truly asserted, the whole theory of the speaking trumpet, supported as it is by practical experience, would fall to the ground, if it were demonstrable that sound spreads equally in every direction. In windy weather it may often be observed, that the sound of a distant bell varies almost instantaneously in its strength, so as to appear at least twice as remote at one time as at another; an observation which has also occurred to another gentleman, who is uncommonly accurate in examining the phænomena of nature. Now, if sound diverged equally in all directions, the variation produced by the wind could never exceed one-tenth of the apparent distance: but, on the supposition of a motion nearly rectilinear, it may easily happen that a slight change in the direction of the wind, may convey the sound, either directly or after reflection, in very different degrees of strength, to the same spot. From the experiments on the motion of a current of air, already related, it would be expected that a sound, admitted at a considerable distance from its origin through an aperture, would proceed, with an almost imperceptible increase of divergence, in the same direction; for, the actual velocity of the particles of air, in the strongest sound, is incomparably less than that of the slowest of the currents in the experiments related, where the beginning of the conical divergence took place at the greatest distance. Dr. MATTHEW YOUNG has objected, not without reason, to M. HUBE, that the existence of a condensation will cause a divergence in sound: but a much greater degree of condensation must have existed in the currents described than in any sound. There is indeed one difference between a stream of air and a sound; that, in sound, the motions of different particles of air are not synchro-

nous: but it is not demonstrable that this circumstance would affect the divergency of the motion, except at the instant of its commencement, and perhaps not even then in a material degree; for, in general, the motion is communicated with a very gradual increase of intensity. The subject, however, deserves a more particular investigation; and, in order to obtain a more solid foundation for the argument, it is proposed, as soon as circumstances permit, to institute a course of experiments for ascertaining, as accurately as possible, the different strength of a sound once projected in a given direction, at different distances from the axis of its motion.

VII. *Of the Decay of Sound.*

Various opinions have been entertained respecting the decay of sound. M. DE LA GRANGE has published a calculation, by which its force is shown to decay nearly in the simple ratio of the distances; and M. DANIEL BERNOULLI's equations for the sounds of conical pipes lead to a similar conclusion. The same inference would follow from a completion of the reasoning of Dr. HELSHAM, Dr. MATTHEW YOUNG, and Professor VENTURI. It has been very elegantly demonstrated by MACLAURIN, and may also be proved in a much more simple manner, that when motion is communicated through a series of elastic bodies increasing in magnitude, if the number of bodies be supposed infinitely great, and their difference infinitely small, the motion of the last will be to that of the first in the subduplicate ratio of their respective magnitudes; and since, in the case of concentric spherical laminæ of air, the bulk increases in the duplicate ratio of the distance, the motion will in this case be directly, and the velocity inversely, as the distance. But, however true this may

be of the first impulse, it will appear, by pursuing the calculation a little further, that every one of the elastic bodies, except the last, receives an impulse in a retrograde direction, which ultimately impedes the effect of the succeeding impulse, as much as a similar cause promoted that of the preceding one: and thus, as sound must be conceived to consist of an infinite number of impulses, the motion of the last lamina will be precisely equal to that of the first; and, as far as this mode of reasoning goes, sound must decay in the duplicate ratio of the distance. Hence it appears, that the proposal for adopting the logarithmic curve for the form of the speaking trumpet, was founded on fallacious reasoning. The calculation of M. De la Grange is left for future examination; and it is intended, in the mean time, to attempt to ascertain the decay of sound as nearly as possible by experiment: should the result favour the conclusions from that calculation, it would establish a marked difference between the propagation of sound and of light.

VIII. *Of the harmonic Sounds of Pipes.*

In order to ascertain the velocity with which organ pipes of different lengths require to be supplied with air, according to the various appropriate sounds which they produce, a set of experiments was made, with the same mouth-piece, on pipes of the same bore, and of different lengths, both stopped and open. The general result was, that a similar blast produced as nearly the same sound as the length of the pipes would permit; or at least that the exceptions, though very numerous, lay equally on each side of this conclusion. The particular results are expressed in Table xi. and in Plate IV. Fig. 28. They explain how a note may be made much louder on a wind instrument

by a swell, than it can possibly be by a sudden impression of the blast. It is proposed, at a future time, to ascertain by experiment, the actual compression of the air within the pipe under different circumstances: from some very slight trials, it seemed to be nearly in the ratio of the frequency of vibrations of each harmonic.

Table XI.

OPEN.						STOPPED.					
A	B	C	D	E	F	A	B	C	D	E	F
4.5		0.7	8.8	$\equiv$ d^*	1	4.5		0.3	1.8	$=$ d	1
	4.1	8.8			2		1.2	1.7	10.0		3
							5.0	9.0			5
9.4		0.3	0.9	$=$ f	1						
	0.8		8.0		2	9.4		0.2	0.4	$-$ f	1
	2.0		18.0		3			0.45	1.6		3
	5.0	8.6	20.0		4		1.1	1.6	8.5		5
	16.5	18.0			5		7.0	8.0			7
	19.0	20.0			6						
						16.1		0.4	0.6	$\equiv$ d^*	3
16.1		0.4	1.0	$=$ g^*	2		0.6	0.65	1.1		5
	0.8	1.0	2.2		3		0.9	1.1	2.4		7
	1.2	2.2	4.7		4		1.6	2.4	4.9		9
	2.2	4.7	11.5		5		2.5	4.8	9.0		11
	3.4		13.5		6		6.0	7.0			13
	4.0		15.0		7						
	6.5	10.0			8						
						20.5		0.8	1.1	$\equiv$ c^*	7
				$=$			1.0	1.1	3.8		9
20.5		0.6	0.8	b	3		1.8		3.8		11
		0.8	1.9		4		3.2	3.8	12.		17
	1.1	1.9	5.7		5			12.		0	00
	4.5	5.7			8						

A, is the length of the pipe from the lateral orifice to the end. C, the pressure at which the sound began. B, its termination, by lessening the pressure; D, by increasing it. E, the note answering to the first sound of each pipe, according to the German method of notation. F, the number showing the place of each note in the regular series of harmonics. The diameter of the pipe was .35; the air duct of the mouth-piece measured, where smallest, .25 by .035; the lateral orifice .25 by .125. The apparatus was not calculated to apply a pressure of above 22 inches. Where no number stands under C, a sudden blast was required to produce the note.

IX. *Of the Vibrations of different elastic Fluids.*

All the methods of finding the velocity of sound, agree in determining it to be, in fluids of a given elasticity, reciprocally in the subduplicate ratio of the density: hence, in pure hydrogen gas it should be $\sqrt{13} = 3.6$ times as great as in common air; and the pitch of a pipe should be a minor fourteenth higher in this fluid than in the common air. It is therefore probable that the hydrogen gas used in Professor CHLADNI's late experiments, was not quite pure. It must be observed, that in an accurate experiment of this nature, the pressure causing the blast ought to be carefully ascertained. There can be no doubt but that, in the observations of the French Academicians on the velocity of sound, which appear to have been conducted with all possible attention, the dampness and coldness of the night air must have considerably increased its density: hence, the velocity was found to be only 1109 feet in a second; while DERHAM's experiments, which have an equal appearance of accuracy, make it amount to 1142. Perhaps the average may, as has been already mentioned, be safely estimated at 1130. It may here be remarked, that the well known elevation of the pitch of wind instruments, in the course of playing, sometimes amounting to half a note, is not, as is commonly supposed, owing to any expansion of the instrument, for this should produce a contrary effect, but to the increased warmth of the air in the tube. Dr. SMITH has made a similar observation, on the pitch of an organ in summer and winter, which he found to differ more than twice as much as the English and French experiments on the velocity of sound. BIANCONI found the velocity of sound, at Bologna, to differ at different times, in the ratio of 152 to 157.

X. *Of the Analogy between Light and Sound.*

Ever since the publication of Sir ISAAC NEWTON's incomparable writings, his doctrines of the emanation of particles of light from lucid substances, and of the formal pre-existence of coloured rays in white light, have been almost universally admitted in this country, and but little opposed in others. LEONARD EULER indeed, in several of his works, has advanced some powerful objections against them, but not sufficiently powerful to justify the dogmatical reprobation with which he treats them; and he has left that system of an ethereal vibration, which after HUYGENS and some others he adopted, equally liable to be attacked on many weak sides. Without pretending to decide positively on the controversy, it is conceived that some considerations may be brought forwards, which may tend to diminish the weight of objections to a theory similar to the HUYGENIAN. There are also one or two difficulties in the NEWTONIAN system, which have been little observed. The first is, the uniform velocity with which light is supposed to be projected from all luminous bodies, in consequence of heat, or otherwise. How happens it that, whether the projecting force is the slightest transmission of electricity, the friction of two pebbles, the lowest degree of visible ignition, the white heat of a wind furnace, or the intense heat of the sun itself, these wonderful corpuscles are always propelled with one uniform velocity? For, if they differed in velocity, that difference ought to produce a different refraction. But a still more insuperable difficulty seems to occur, in the partial reflection from every refracting surface. Why, of the same kind of rays, in every circumstance precisely similar, some should always be reflected,

and others transmitted, appears in this system to be wholly inexplicable. That a medium resembling, in many properties, that which has been denominated ether, does really exist, is undeniably proved by the phænomena of electricity; and the arguments against the existence of such an ether throughout the universe, have been pretty sufficiently answered by EULER. The rapid transmission of the electrical shock, shows that the electric medium is possessed of an elasticity as great as is necessary to be supposed for the propagation of light. Whether the electric ether is to be considered as the same with the luminous ether, if such a fluid exists, may perhaps at some future time be discovered by experiment; hitherto I have not been able to observe that the refractive power of a fluid undergoes any change by electricity. The uniformity of the motion of light in the same medium, which is a difficulty in the NEWTONIAN theory, favours the admission of the HUYGENIAN; as all impressions are known to be transmitted through an elastic fluid with the same velocity. It has been already shown, that sound, in all probability, has very little tendency to diverge: in a medium so highly elastic as the luminous ether must be supposed to be, the tendency to diverge may be considered as infinitely small, and the grand objection to the system of vibration will be removed. It is not absolutely certain, that the white line visible in all directions on the edge of a knife, in the experiments of NEWTON and of Mr. JORDAN, was not partly occasioned by the tendency of light to diverge. EULER's hypothesis, of the transmission of light by an agitation of the particles of the refracting media themselves, is liable to strong objections; according to this supposition, the refraction of the rays of light, on entering the atmosphere from the pure ether which he describes, ought

to be a million times greater than it is. For explaining the phænomena of partial and total reflection, refraction, and inflection, nothing more is necessary than to suppose all refracting media to retain, by their attraction, a greater or less quantity of the luminous ether, so as to make its density greater than that which it possesses in a vacuum, without increasing its elasticity; and that light is a propagation of an impulse communicated to this ether by luminous bodies: whether this impulse is produced by a partial emanation of the ether, or by vibrations of the particles of the body, and whether these vibrations are, as EULER supposed, of various and irregular magnitudes, or whether they are uniform, and comparatively large, remains to be hereafter determined. Now, as the direction of an impulse transmitted through a fluid, depends on that of the particles in synchronous motion, to which it is always perpendicular, whatever alters the direction of the pulse, will inflect the ray of light. If a smaller elastic body strike against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum. Let AB, Plate V. Fig. 29, be a ray of light falling on the reflecting surface FG; *cd* the direction of the vibration, pulse, impression, or conden-

sation. When *d* comes to H, the impression will be, either wholly or partly, reflected with the same velocity as it arrived, and EH will be equal to DH; the angle EIH to DIH or CIF; and the angle of reflection to that of incidence. Let FG, Fig. 30, be a refracting surface. The portion of the pulse IE, which is travelling through the refracting medium, will move with a greater or less velocity in the subduplicate ratio of the densities, and HE will be to KI in that ratio. But HE is, to the radius IH, the sine of the angle of refraction; and KI that of the angle of incidence. This explanation of refraction is nearly the same as that of EULER. The total reflection of a ray of light by a refracting surface, is explicable in the same manner as its simple refraction; HE, Fig. 31, being so much longer than KI, that the ray first becomes parallel to FG, and then, having to return through an equal diversity of media, is reflected in an equal angle. When a ray of light passes near an inflecting body, surrounded, as all bodies are supposed to be, with an atmosphere of ether denser than the ether of the ambient air, the part of the ray nearest the body is retarded, and of course the whole ray inflected towards the body, Fig. 32. The repulsion of inflected rays has been very ably controverted by Mr. JORDAN, the ingenious author of a late publication on the Inflection of Light. It has already been conjectured by EULER, that the colours of light consist in the different frequency of the vibrations of the luminous ether: it does not appear that he has supported this opinion by any argument; but it is strongly confirmed, by the analogy between the colours of a thin plate and the sounds of a series of organ pipes. The phænomena of the colours of thin plates require, in the NEWTONIAN system, a very complicated supposition, of an ether, anticipating by its

motion the velocity of the corpuscles of light, and thus producing the fits of transmission and reflection; and even this supposition does not much assist the explanation. It appears, from the accurate analysis of the phænomena which NEWTON has given, and which has by no means been superseded by any later observations, that the same colour recurs whenever the thickness answers to the terms of an arithmetical progression. Now this is precisely similar to the production of the same sound, by means of an uniform blast, from organ-pipes which are different multiples of the same length. Supposing white light to be a continued impulse or stream of luminous ether, it may be conceived to act on the plates as a blast of air does on the organ-pipes, and to produce vibrations regulated in frequency by the length of the lines which are terminated by the two refracting surfaces. It may be objected that, to complete the analogy, there should be tubes, to answer to the organ-pipes: but the tube of an organ-pipe is only necessary to prevent the divergence of the impression, and in light there is little or no tendency to diverge; and indeed, in the case of a resonant passage, the air is not prevented from becoming sonorous by the liberty of lateral motion. It would seem, that the determination of a portion of the track of a ray of light through any homogeneous stratum of ether, is sufficient to establish a length as a basis for colorific vibrations. In inflections, the length of the track of a ray of light through the inflecting atmosphere may determine its vibrations: but, in this case, as it is probable that there is a reflection from every part of the surface of the surrounding atmosphere, contributing to the appearance of the white line in every direction, in the experiments already mentioned, so it is possible that there may be some second reflection

at the immediate surface of the body itself, and that, by mutual reflections between these two surfaces, something like the anguiform motion suspected by NEWTON may really take place; and then the analogy to the colours of thin plates will be still stronger. A mixture of vibrations, of all possible frequencies, may easily destroy the peculiar nature of each, and concur in a general effect of white light. The greatest difficulty in this system is, to explain the different degree of refraction of differently coloured light, and the separation of white light in refraction: yet, considering how imperfect the theory of elastic fluids still remains, it cannot be expected that every circumstance should at once be clearly elucidated. It may hereafter be considered how far the excellent experiments of Count RUMFORD, which tend very greatly to weaken the evidence of the modern doctrine of heat, may be more or less favourable to one or the other system of light and colours. It does not appear that any comparative experiments have been made on the inflection of light by substances possessed of different refractive powers; undoubtedly some very interesting conclusions might be expected from the inquiry.

XI. *Of the Coalescence of musical Sounds.*

It is surprising that so great a mathematician as Dr. SMITH could have entertained for a moment, an idea that the vibrations constituting different sounds should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise effected than by each particle's partaking of both motions. If this assertion stood in need of any proof, it might be amply

furnished by the phænomena of beats, and of the grave harmonics observed by Romieu and Tartini; which M. De la Grange has already considered in the same point of view. In the first place, to simplify the statement, let us suppose, what probably never precisely happens, that the particles of air, in transmitting the pulses, proceed and return with uniform motions; and, in order to represent their position to the eye, let the uniform progress of time be represented by the increase of the absciss, and the distance of the particle from its original position, by the ordinate, Fig. 33—38. Then, by supposing any two or more vibrations in the same direction to be combined, the joint motion will be represented by the sum or difference of the ordinates. When two sounds are of equal strength, and nearly of the same pitch, as in Fig. 36, the joint vibration is alternately very weak and very strong, producing the effect denominated a beat, Plate VI. Fig. 43, B and C; which is slower and more marked, as the sounds approach nearer to each other in frequency of vibrations; and, of these beats there may happen to be several orders, according to the periodical approximations of the numbers expressing the proportions of the vibrations. The strength of the joint sound is double that of the simple sound only at the middle of the beat, but not throughout its duration; and it may be inferred, that the strength of sound in a concert will not be in exact proportion to the number of instruments composing it. Could any method be devised for ascertaining this by experiment, it would assist in the comparison of sound with light. In Plate V. Fig. 33, let P and Q be the middle points of the progress or regress of a particle in two successive compound vibrations; then, CP being = PD, KR = RN, GQ = QH, and MS = SO, twice their distance, 2RS = 2RN +

$2NM + 2MS = KN + NM + NM + MO = KM + NO$, is equal to the sum of the distances of the corresponding parts of the simple vibrations. For instance, if the two sounds be as 80 : 81, the joint vibration will be as 80.5; the arithmetical mean between the periods of the single vibrations. The greater the difference in the pitch of two sounds, the more rapid the beats, till at last, like the distinct puffs of air in the experiments already related, they communicate the idea of a continued sound; and this is the fundamental harmonic described by TARTINI. For instance, in Plate V. Fig. 34—37, the vibrations of sounds related as 1 : 2, 4 : 5, 9 : 10, and 5 : 8, are represented; where the beats, if the sounds be not taken too grave, constitute a distinct sound, which corresponds with the time elapsing between two successive coincidences, or near approaches to coincidence: for, that such a tempered interval still produces a harmonic, appears from Plate V. Fig. 38. But, besides this primary harmonic, a secondary note is sometimes heard, where the intermediate compound vibrations occur at a certain interval, though interruptedly; for instance, in the coalescence of two sounds related to each other as 7 : 8, 5 : 7, or 4 : 5, there is a recurrence of a similar state of the joint motion, nearly at the interval of $\frac{1}{15}$, $\frac{1}{12}$, or $\frac{1}{3}$ of the whole period: hence, in the concord of a major third, the fourth below the key note is heard as distinctly as the double octave, as is seen in some degree in Plate V. Fig. 35; AB being nearly two-thirds of CD. The same sound is sometimes produced by taking the minor sixth below the key note; probably because this sixth, like every other note, is almost always attended by an octave, as a harmonic. If the angles of all the figures resulting from the motion thus assumed be rounded off, they will approach more nearly

to a representation of the actual circumstances; but, as the laws by which the motion of the particles of air is regulated, differ according to the different origin and nature of the sound, it is impossible to adapt a demonstration to them all: if, however, the particles be supposed to follow the law of the harmonic curve, derived from uniform circular motion, the compound vibration will be the harmonic instead of the arithmetical mean; and the secondary sound of the interrupted vibrations will be more accurately formed, and more strongly marked, Plate VI. Figs. 41, 42: the demonstration is deducible from the properties of the circle. It is remarkable, that the law by which the motion of the particles is governed, is capable of some singular alterations by a combination of vibrations. By adding to a given sound other similar sounds, related to it in frequency as the series of odd numbers, and in strength inversely in the same ratios, the right lines indicating an uniform motion may be converted very nearly into figures of sines, and the figures of sines into right lines, as in Plate V. Figs. 39, 40.

XII. *Of the Frequency of Vibrations constituting a given Note.*

The number of vibrations performed by a given sound in a second, has been variously ascertained: first, by SAUVEUR, by a very ingenious inference from the beats of two sounds; and since, by the same observer and several others, by calculation from the weight and tension of a chord. It was thought worth while, as a confirmation, to make an experiment suggested, but coarsely conducted, by MERSENNUS, on a chord 200 inches in length, stretched so loosely as to have its single vibrations visible; and, by holding a quill nearly in contact with the chord,

they were made audible, and were found, in one experiment, to recur 8.3 times in a second. By lightly pressing the chord at one-eighth of its length from the end, and at other shorter aliquot distances, the fundamental note was found to be one-sixth of a tone higher than the respective octave of a tuning-fork marked C: hence, the fork was a comma and a half above the pitch assumed by SAUVEUR, of an imaginary C, consisting of one vibration in a second.

XIII. *Of the Vibrations of Chords.*

By a singular oversight in the demonstration of Dr. BROOK TAYLOR, adopted as it has been by a number of later authors, it is asserted, that if a chord be once inflected into any other form than that of the harmonic curve, it will, since those parts which are without this figure are impelled towards it by an excess of force, and those within it by a deficiency, in a very short time arrive at or very near the form of this precise curve. It would be easy to prove, if this reasoning were allowed, that the form of the curve can be no other than that of the axis, since the tending force is continually impelling the chord towards this line. The case is very similar to that of the NEWTONIAN proposition respecting sound. It may be proved, that every impulse is communicated along a tended chord with an uniform velocity; and this velocity is the same which is inferred from Dr. TAYLOR's theorem; just as that of sound, determined by other methods, coincides with the NEWTONIAN result. But, although several late mathematicians have given admirable solutions of all possible cases of the problem, yet it has still been supposed, that the distinctions were too minute to be actually observed; especially, as it might have been added, since

the inflexibility of a wire would dispose it, according to the doctrine of elastic rods, to assume the form of the harmonic curve. The theorem of EULER and DE LA GRANGE, in the case where the chord is supposed to be at first at rest, is in effect this: continue the figure each way, alternately on different sides of the axis, and in contrary positions; then, from any point of the curve, take an abscis each way, in the same proportion to the length of the chord as any given portion of time bears to the time of one semivibration, and the half sum of the ordinates will be the distance of that point of the chord from the axis, at the expiration of the time given. If the initial figure of the chord be composed of two right lines, as generally happens in musical instruments and experiments, its successive forms will be such as are represented in Plate VI. Figs. 47, 48: and this result is fully confirmed by experiment. Take one of the lowest strings of a square piano forte, round which a fine silvered wire is wound in a spiral form; contract the light of a window, so that, when the eye is placed in a proper position, the image of the light may appear small, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path, like a burning coal whirled round, and will present to the eye a line of light, which, by the assistance of a microscope, may be very accurately observed. According to the different ways by which the wire is put in motion, the form of this path is no less diversified and amusing, than the multifarious forms of the quiescent lines of vibrating plates, discovered by Professor CHLADNI; and is indeed in one respect even more interesting, as it appears to be more within the reach of mathematical calculation to determine it; although hitherto, excepting some slight observations of BUSSE

and CHLADNI, principally on the motion of rods, nothing has been attempted on the subject. For the present purpose, the motion of the chord may be simplified, by tying a long fine thread to any part of it, and fixing this thread in a direction perpendicular to that of the chord, without drawing it so tight as to increase the tension: by these means, the vibrations are confined nearly to one plane, which scarcely ever happens when the chord vibrates at liberty. If the chord be now inflected in the middle, it will be found, by comparison with an object which marked its quiescent position, to make equal excursions on each side of the axis; and the figure which it apparently occupies will be terminated by two lines, the more luminous as they are nearer the ends, Plate VI. Fig. 49. But, if the chord be inflected near one of its extremities, Fig. 50, it will proceed but a very small distance on the opposite side of the axis, and will there form a very bright line, indicating its longer continuance in that place; yet it will return on the former side nearly to the point from whence it was let go, but will be there very faintly visible, on account of its short delay. In the middle of the chord, the excursions on each side the axis are always equal; and, beyond the middle, the same circumstances take place as in the half where it was inflected, but on the opposite side of the axis; and this appearance continues unaltered in its proportions, as long as the chord vibrates at all: fully confirming the non-existence of the harmonic curve, and the accuracy of the construction of EULER and DE LA GRANGE. At the same time, as M. BERNOULLI has justly observed, since every figure may be infinitely approximated, by considering its ordinates as composed of the ordinates of an infinite number of trochoids of different magnitudes, it may be demonstrated, that

all these constituent curves would revert to their initial state, in the same time that a similar chord bent into a trochoidal curve would perform a single vibration; and this is in some respects a convenient and compendious method of considering the problem. But, when a chord vibrates freely, it never remains long in motion, without a very evident departure from the plane of the vibration; and, whether from the original obliquity of the impulse, or from an interference with the reflected vibrations of the air, or from the inequability of its own weight or flexibility, or from the immediate resistance of the particles of air in contact with it, it is thrown into a very evident rotatory motion, more or less simple and uniform according to circumstances. Some specimens of the figures of the orbits of chords are exhibited in Plate VI. Fig. 44. At the middle of the chord, its orbit has always two equal halves, but seldom at any other point. The curves of Fig. 46, are described by combining together various circular motions, supposed to be performed in aliquot parts of the primitive orbit: and some of them approach nearly to the figures actually observed. When the chord is of unequal thickness, or when it is loosely tended and forcibly inflected, the apsides and double points of the orbits have a very evident rotatory motion. The compound rotations seem to demonstrate to the eye the existence of secondary vibrations, and to account for the acute harmonic sounds which generally attend the fundamental sound. There is one fact respecting these secondary notes, which seems intirely to have escaped observation. If a chord be inflected at one-half, one-third, or any other aliquot part of its length, and then suddenly left at liberty, the harmonic note which would be produced by dividing the chord at that point is intirely lost, and is not to be dis-

tinguished during any part of the continuance of the sound. This demonstrates, that the secondary notes do not depend upon any interference of the vibrations of the air with each other, nor upon any sympathetic agitation of auditory fibres, nor upon any effect of reflected sound upon the chord, but merely upon its initial figure and motion. If it were supposed that the chord, when inflected into right lines, resolved itself necessarily into a number of secondary vibrations, according to some curves which, when properly combined, would approximate to the figure given, the supposition would indeed in some respects correspond with the phænomenon related; as the coefficients of all the curves supposed to end at the angle of inflection would vanish. But, whether we trace the constituent curves of such a figure through the various stages of their vibrations, or whether we follow the more compendious method of EULER to the same purpose, the figures resulting from this series of vibrations are in fact so simple, that it seems inconceivable how the ear should deduce the complicated idea of a number of heterogeneous vibrations, from a motion of the particles of air which must be extremely regular, and almost uniform; an uniformity which, when proper precautions are taken, is not contradicted by examining the motion of the chord with the assistance of a powerful magnifier. This difficulty occurred very strongly to EULER; and DE LA GRANGE even suspects some fallacy in the experiment, and that a musical ear judges from previous association. But, besides that these sounds are discoverable to an ear destitute of such associations, and, when the sound is produced by two strings in imperfect unison, may be verified by counting the number of their beats, the experiment already related is an undeniable proof that no fallacy

of this kind exists. It must be confessed, that nothing fully satisfactory has yet occurred to account for the phænomena; but it is highly probable that the slight increase of tension produced by flexure, which is omitted in the calculations, and the unavoidable inequality of thickness or flexibility of different parts of the same chord, may, by disturbing the isochronism of the subordinate vibrations, cause all that variety of sounds which is so inexplicable without them. For, when the slightest difference is introduced in the periods, there is no difficulty in conceiving how the sounds may be distinguished; and indeed, in some cases, a nice ear will discover a slight imperfection in the tune of harmonic notes: it is also often observed, in tuning an instrument, that some of the single chords produce beating sounds, which undoubtedly arise from their want of perfect uniformity. It may be perceived that any particular harmonic is loudest, when the chord is inflected at about one-third of the corresponding aliquot part from one of the extremities of that part. An observation of Dr. WALLIS seems to have passed unnoticed by later writers on harmonics. If the string of a violin be struck in the middle, or at any other aliquot part, it will give either no sound at all, or a very obscure one. This is true, not of inflection, but of the motion communicated by a bow; and may be explained from the circumstance of the successive impulses, reflected from the fixed points at each end, destroying each other: an explanation nearly analogous to some observations of Dr. MATTHEW YOUNG on the motion of chords. When the bow is applied not exactly at the aliquot point, but very near it, the corresponding harmonic is extremely loud; and the fundamental note, especially in the lowest harmonics, scarcely audible: the chord assumes the appearance, at the

aliquot points, of as many lucid lines as correspond to the number of the harmonic, more nearly approaching to each other as the bow approaches more nearly to the point, Plate VI. Fig. 51. According to the various modes of applying the bow, an immense variety of figures of the orbits are produced, Fig. 45, more than enough to account for all the difference of tone in different performers. In observations of this kind, a series of harmonics is frequently heard in drawing the bow across the same part of the chord: these are produced by the bow; they are however not proportionate to the whole length of the bow, but depend on the capability of the portion of the bowstring, intercepted between its end and the chord, of performing its vibrations in times which are aliquot parts of the vibration of the chord: hence it would seem, that the bow takes effect on the chord but at one instant during each fundamental vibration. In these experiments, the bow was strung with the second string of a violin: and, in the preparatory application of resin, the longitudinal sound of CHLADNI was sometimes heard; but it was observed to differ at least a note in different parts of the string.

XIV. *Of the Vibrations of Rods and Plates.*

Some experiments were made, with the assistance of a most excellent practical musician, on the various notes produced by a glass tube, an iron rod, and a wooden ruler; and, in a case where the tube was as much at liberty as possible, all the harmonics corresponding to the numbers from 1 to 13, were distinctly observed; several of them at the same time, and others by means of different blows. This result seems to differ from the calculations of EULER and Count RICCATI, confirmed as

they are by the repeated experiments of Professor CHLADNI; it is not therefore brought forward as sufficiently controverting those calculations, but as showing the necessity of a revision of the experiments. Scarcely any note could ever be heard when a rod was loosely held at its extremity; nor when it was held in the middle, and struck one-seventh of the length from one end. The very ingenious method of Professor CHLADNI, of observing the vibrations of plates by strewing fine sand over them, and discovering the quiescent lines by the figures into which it is thrown, has hitherto been little known in this country: his treatise on the phænomena is so complete, that no other experiments of the kind were thought necessary. Glass vessels of various descriptions, whether made to sound by percussion or friction, were found to be almost intirely free from harmonic notes; and this observation coincides with the experiments of CHLADNI.

XV. *Of the human Voice.*

The human voice, which was the object originally proposed to be illustrated by these researches, is of so complicated a nature, and so imperfectly understood, that it can be on this occasion but superficially considered. No person, unless we except M. FERREIN, has published any thing very important on the subject of the formation of the voice, before or since DODART; his reasoning has fully shown the analogy between the voice and the *voix humaine* and regal organ-pipes: but his comparison with the whistle is unfortunate; nor is he more happy in his account of the falsetto. A kind of experimental analysis of the voice may be thus exhibited. By drawing in the breath, and at the same time properly contracting the larynx, a slow vibration of the ligaments of the glottis may be produced, making a distinct clicking sound:

upon increasing the tension, and the velocity of the breath, this clicking is lost, and the sound becomes continuous, but of an extremely grave pitch: it may, by a good ear, be distinguished two octaves below the lowest A of a common bass voice, consisting in that case of about 26 vibrations in a second. The same sound may be raised nearly to the pitch of the common voice; but it is never smooth and clear, except perhaps in some of those persons called ventriloquists. When the pitch is raised still higher, the upper orifice of the larynx, formed by the summits of the arytænoid cartilages and the epiglottis, seems to succeed to the office of the ligaments of the glottis, and to produce a retrograde falsetto, which is capable of a very great degree of acuteness. The same difference probably takes place between the natural voice and the common falsetto: the rimula glottidis being too long to admit of a sufficient degree of tension for very acute sounds, the upper orifice of the larynx supplies its place; hence, taking a note within the compass of either voice, it may be held, with the same expanse of air, two or three times as long in a falsetto as in a natural voice; hence, too, the difficulty of passing smoothly from the one voice to the other. It has been remarked, that the larynx is always elevated when the sound is acute: but this elevation is only necessary in rapid transitions, as in a shake; and then probably because, by the contraction of the capacity of the trachea, an increase of the pressure of the breath can be more rapidly effected this way, than by the action of the abdominal muscles alone. The reflection of the sound thus produced from the various parts of the cavity of the mouth and nostrils, mixing at various intervals with the portions of the vibrations directly proceeding from the larynx, must, according to the temporary form of the parts, variously affect the laws of the motion of the air in each vibra-

tion, or, according to EULER's expression, the equation of the curve conceived to correspond with this motion, and thus produce the various characters of the vowels and semi-vowels. The principal sounding board seems to be the bony palate: the nose, except in nasal letters, affords but little resonance; for the nasal passage may be closed, by applying the finger to the soft palate, without much altering the sound of vowels not nasal. A good ear may distinctly observe, especially in a loud bass voice, besides the fundamental note, at least four harmonic sounds, in the order of the natural numbers; and, the more reedy the tone of the voice, the more easily they are heard. Faint as they are, their origin is by no means easy to be explained. This observation is precisely confirmed, in a late dissertation of M. KNECHT, published in the musical newspaper of Leipsic. Perhaps, by a close attention to the harmonics entering into the constitution of various sounds, more may be done in their analysis than could otherwise be expected.

XVI. *Of the Temperament of musical Intervals.*

It would have been extremely convenient for practical musicians, and would have saved many warm controversies among theoretical ones, if three times the ratio of 4 to 5, or four times that of 5 to 6, had been equal to the ratio of 1 to 2. As it happens to be otherwise, it has been much disputed in what intervals the imperfection should be placed. The ARISTOXENIANS and PYTHAGOREANS were in some sense the beginners of the controversy. SAUVEUR has given very comprehensive tables of a great number of systems of temperament; and his own now ranks among the many that are rejected. Dr. SMITH has written a large and obscure volume, which, for every purpose but for

the use of an impracticable instrument, leaves the whole subject precisely where it found it. KIRNBERGER, MARPURG, and other German writers, have disputed with great bitterness, almost every one for a particular method of tuning. It is not with any confidence of success, that one more attempt is made, which rests its chief claim to preference, on the similarity of its theory to the actual practice of the best instrument-makers. However we estimate the degree of imperfection of two tempered concords of the same nature, it will appear, that the manner of dividing the temperament between them does not materially alter its aggregate sum; for instance, the imperfection of a comma in a major-third, occasions it to beat very nearly twice as fast as that of half a comma. If indeed the imperfection were great, it might affect an interval so materially as to destroy its character; as, in some methods of temperament, a minor third diminished by two commas approaches more nearly to the ratio 6 : 7, than to 5 : 6; but, with this limitation, the sum of harmony is nearly equal in all systems. Hence, if every one of the twelve major and minor thirds occurred equally often in the compositions which are to be performed on an instrument, it would be of no great consequence, to the sum of the imperfections, among which of the thirds they were divided: and, even in this case, the opinion of the best practical authors is, that the difference of character produced by a difference of proportions in various keys, would be of considerable advantage in the general effect of modulation. But, when it is considered, that upon an average of all the music ever composed, some particular keys occur at least twice as often as others, there seems to be a very strong additional reason for making the harmony the most perfect in those keys which are the most frequently

used; since the aggregate sum of all the imperfections which occur in playing, must by this means be diminished in the greatest possible degree, and the diversity of character at the same time preserved. Indeed, in practice, this method, under different modifications, has been almost universal; for, although many have pretended to an equal temperament, yet the methods which they have employed to attain it have been evidently defective. It appears to me, that every purpose may be answered, by making C : E too sharp by a quarter of a comma, which will not offend the nicest ear; E : G*, and A♭ : C, equal; F* : A* too sharp by a comma; and the major thirds of all the intermediate keys more or less perfect, as they approach more or less to C in the order of modulation. The fifths are perfect enough in every system. The results of this method are shown in Table XII. In practice, nearly the same effect may be very simply produced, by tuning from C to F, B♭, E♭, G*, C*, F* six perfect fourths; and C, G, D, A, E, B, F*, six equally imperfect fifths, Plate VI. Fig. 52. If the unavoidable imperfections of the fourths be such as to incline them to sharpness, the temperament will approach more nearly to equality, which is preferable to an inaccuracy on the other side. An easy method of comparing different systems of temperament is exhibited in Plate VII. Fig. 53, which may easily be extended to all the systems that have ever been invented.

Table XII.

A		B		C	
C	50000	1 C	+ .0013487	1 A, E	— .0028603
B	53224	2 G, F	.0019006	2 D, B	.0029122
B♭	56131	3 D, B♭	.0024525	3 G, F*	.0034641
A	59676	4 A, E♭	.0034641	4 C, C*	.0044756
G*	63148	5 E, A♭	.0044756	5 F, G*	.0049353
G	66822	6 B, C*	.0049353	6 B♭, E♭	.0053950
F*	71041	7 F*	.0053950		
F	74921				
E	79752	D			
E♭	89810				
D	89304	1 E♭, G*, C*, F*	— .0000000		
C*	94723	2 F, B♭, E, B	.0004597		
C	100000	3 C, G, D, A	.0011562		

A, shows the division of a monochord corresponding to each note, in the system proposed. B, the logarithm of the temperament of each of the major thirds. C, of the minor thirds. D, of the fifths; C and D being both negative.

Thus, Sir, I have endeavoured to advance a few steps only, in the investigation of some very obscure but interesting subjects. As far as I know, most of these observations are new; but, if they should be found to have been already made by any other person, their repetition in a connected chain of inference may still be excusable. I am persuaded also, that at least some of the positions maintained are incontrovertibly consistent with truth and nature; but, should further experiments tend to confute any opinions that I have suggested, I shall relinquish them with as much readiness as I have long since abandoned the

hypothesis which I once took the liberty of submitting to the Royal Society, on the functions of the crystalline lens.

I am, &c.

Emanuel College, Cambridge,
8th July, 1799.

THOMAS YOUNG.

EXPLANATION OF THE FIGURES.

(See Plates III. IV. V. VI. and VII.)

Plate III.

Figs. 1—6. The section of a stream of air from a tube .07 inch in diameter, as ascertained by measuring the breadth of the impression on the surface of a liquid. The pressure impelling the current, was in Fig. 1, 1 inch. Fig. 2, 2. Fig. 3, 3. Fig. 4, 4. Fig. 5, 7. Fig. 6, 10.

Figs. 7—12. A similar section, where the tube was .1 in diameter, compared with the section as inferred from the experiments with two gages, which is represented by a dotted line. From this comparison it appears, that where the velocity of the current was small, its central parts only displaced the liquid; and that, where it was great, it displaced, on meeting with resistance, a surface somewhat greater than its own section. The pressure was in Fig. 7, 1. Fig. 8, 2. Fig. 9, 3. Fig. 10, 4. Fig. 11, 7. Fig. 12, 10.

Figs. 13—20. A, the half section of a stream of air from a tube .1 in diameter, as inferred from experiments with two water gages. The pressure was in Fig. 13, 1. Fig. 14, 2. Fig. 15, 5. Fig. 16, 1. Fig. 17, 3. Fig. 18, 5. Fig. 19, 7. Fig. 20, 10. The fine lines, marked B, show the result of the observa-

tions with an aperture .15 in diameter opposed to the stream; C with .3; and D with .5.

Figs. 21—23. A, the half section of a current from a tube .3 in diameter, with a pressure of .5, of 1, and of 3. B shows the course of a portion next the axis of the current, equal in diameter to those represented by the last figures.

Plate IV.

Fig. 24. The appearance of a stream of smoke forced very gently from a fine tube. Fig. 25 and 26, the same appearance when the pressure is gradually increased.

Fig. 27. See Section III.

Fig. 28. The perpendicular lines over each division of the horizontal line show, by their length and distance from that line, the extent of pressure capable of producing, from the respective pipes, the harmonic notes indicated by the figures placed opposite the beginning of each, according to the scale of 22 inches parallel to them. The larger numbers, opposite the middle of each of these lines, show the number of vibrations of the corresponding sound in a second.

Plate V.

Figs. 29—33. See Section X.

Fig. 34. The combination of two equal sounds constituting the interval of an octave, supposing the progress and regress of the particles of air equable. Figs. 35, 36, 37, a similar representation of a major third, major tone, and minor sixth.

Fig. 38. A fourth, tempered about two commas.

Fig. 39. A vibration of a similar nature, combined with subordinate vibrations of the same kind in the ratios of 3, 5, and 7.

Fig. 40. A vibration represented by a curve of which the ordinates are the sines of circular arcs increasing uniformly, corresponding with the motion of a cycloidal pendulum, combined with similar subordinate vibrations in the ratios of 3, 5, and 7.

Plate VI.

Figs. 41 and 42. Two different positions of a major third, composed of similar vibrations, as represented by figures of sines.

Fig. 43. A contracted representation of a series of vibrations. A, a simple uniform sound. B, the beating of two equal sounds nearly in unison, as derived from rectilinear figures. C, the beats of two equal sounds, derived from figures of sines. D, a musical consonance, making by its frequent beats a fundamental harmonic. E, the imperfect beats of two unequal sounds.

Fig. 44. Various forms of the orbit of a musical chord, when inflected, and when struck.

Fig. 45. Forms of the orbit, when the sound is produced by means of a bow.

Fig. 46. Epitrochoidal curves, formed by combining a simple rotation or vibration with other subordinate rotations or vibrations.

Figs. 47 and 48. The successive forms of a tended chord, when inflected and let go, according to the construction of De la Grange and Euler.

Fig. 49. The appearance of a vibrating chord which had been inflected in the middle, the strongest lines representing the most luminous parts.

Fig. 50. The appearance of a vibrating chord, when inflected at any other point than the middle.

Fig. 51. The appearance of a chord, when put in motion by a bow applied nearly at one third of the length from its end.

Fig. 52. The method of tuning recommended for common use.

Plate VII.

Fig. 53. A comparative view of different systems of temperament. The whole circumference represents an octave. The inner circle L is divided into 30103 parts, corresponding with the logarithmical parts of an octave. The next circle R shows the magnitude of the simplest musical and other ratios. Q is divided into twelve equal parts, representing the semitones of the equal temperament described by ZARLINO, differing but little from the system of ARISTOXENUS, and warmly recommended by MARPURG and other late writers. Y exhibits the system proposed in this paper as the most desirable; and P the practical method nearly approaching to it, which corresponds with the eleventh method in MARPURG's enumeration, except that, by beginning with C instead of B, the practical effect of the temperament is precisely inverted. K is the system of KIRNBERGER and SULZER; which is derived from one perfect third, ten perfect and two equally imperfect fifths. M is the system of mean tones, the *sistema participato* of the old Italian writers, still frequently used in tuning organs, approved also by Dr. SMITH for common use. S shows the result of all the calculations in Dr. SMITH's harmonics, the system proposed for his changeable harpsichord, but neither in that nor any other form capable of practical application.

Fig. 1

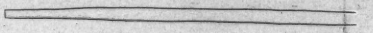


Fig. 2

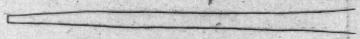
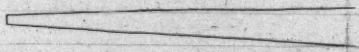


Fig. 3



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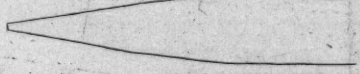

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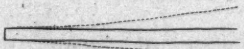

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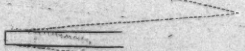

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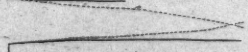

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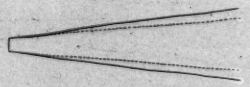

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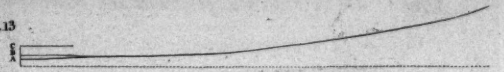

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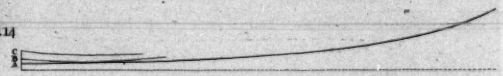

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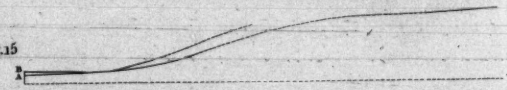

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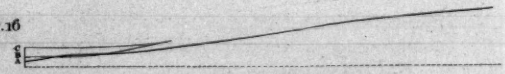

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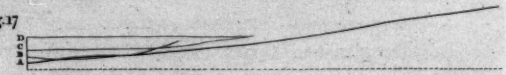

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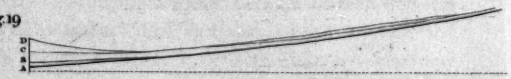

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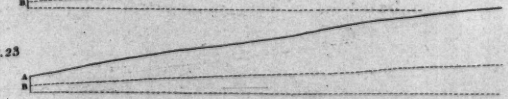

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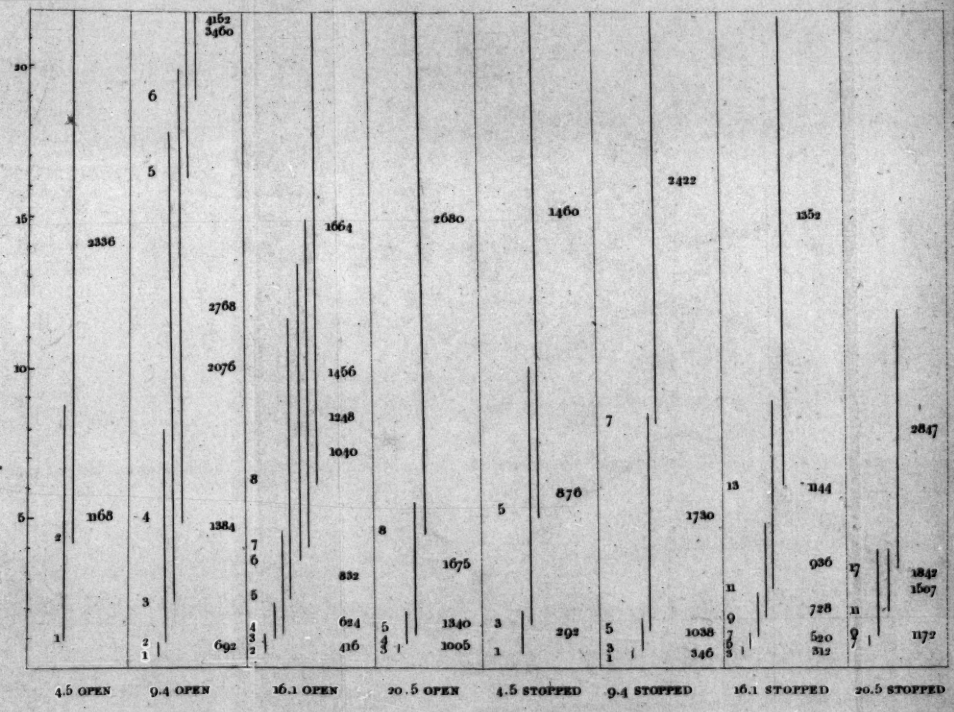

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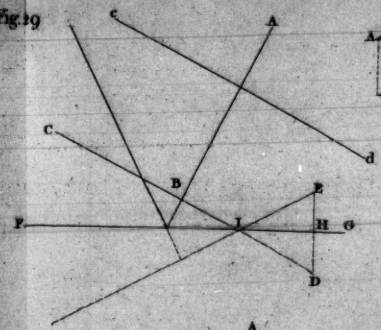

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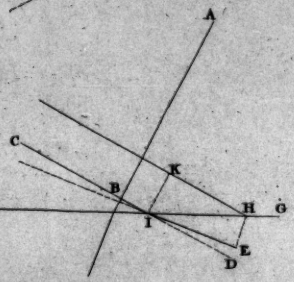

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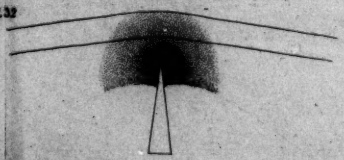

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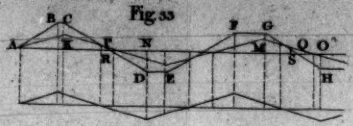

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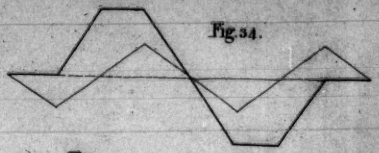

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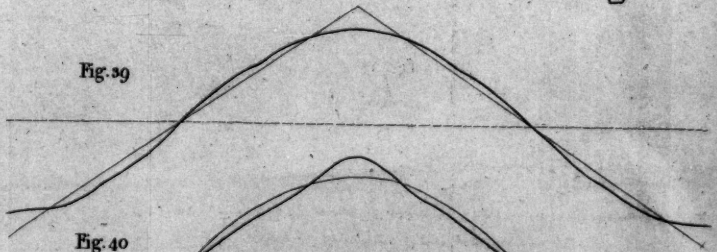

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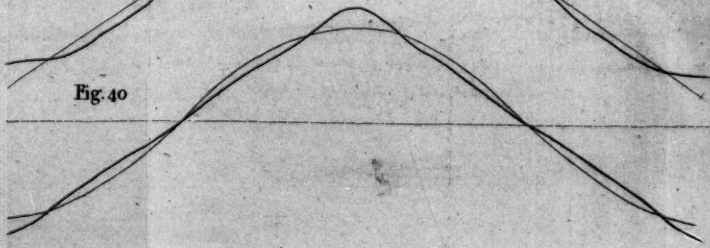

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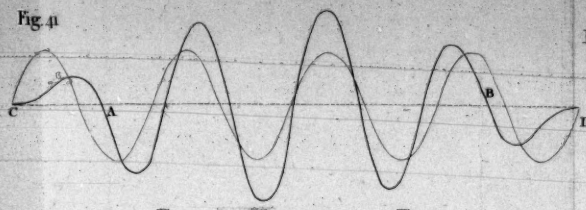

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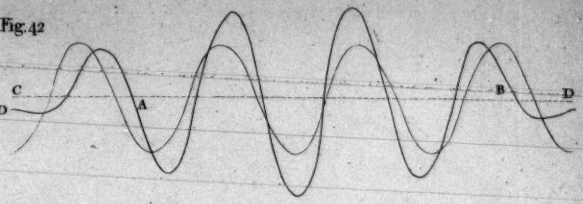

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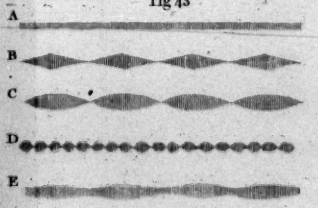

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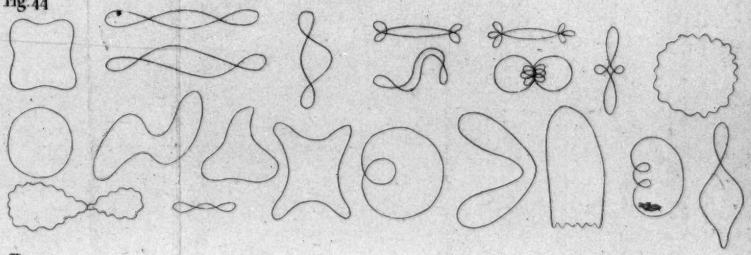

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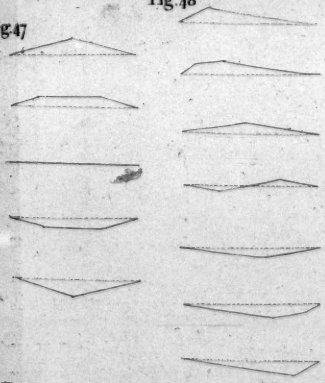

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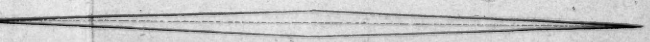

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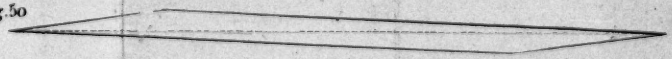

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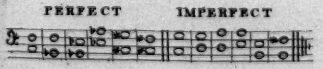

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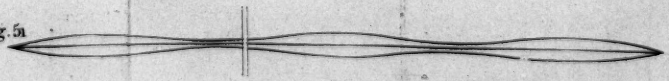

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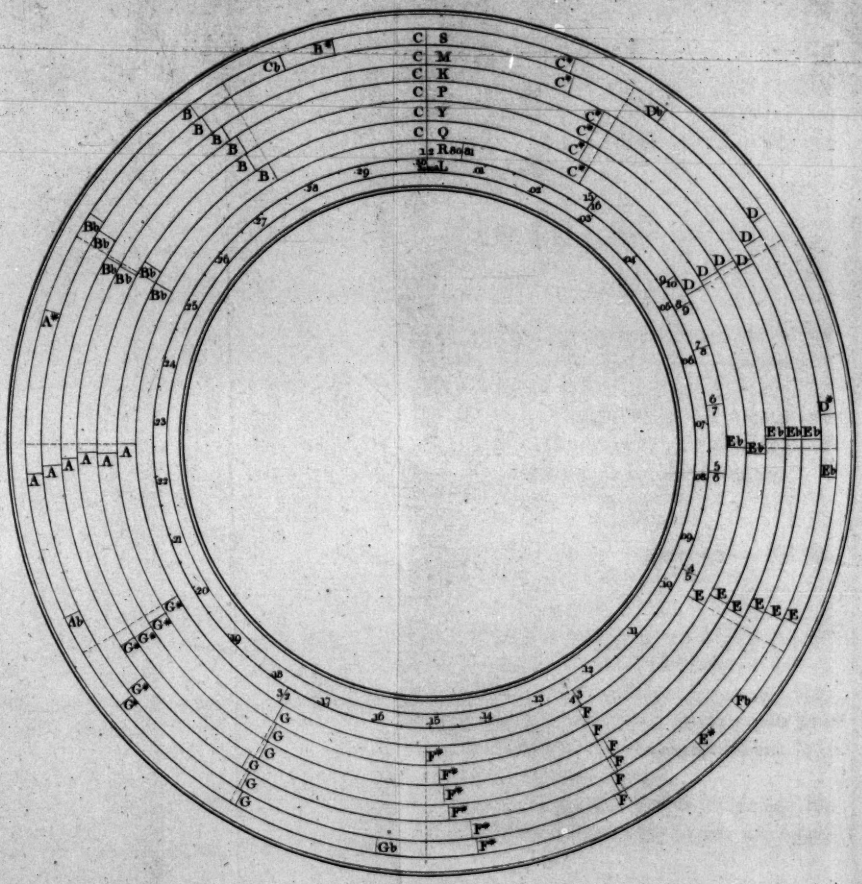

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VIII. *Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear. By Mr.* Astley Cooper. *In a Letter to* Everard Home, *Esq. F. R. S. by whom some Remarks are added.*

Read February 6, 1800.

DEAR SIR,

At the time you were engaged in the investigation of the structure and uses of the membrana tympani, you mentioned a wish to ascertain the effect a rupture of that membrane would have upon hearing. I now send you some observations on that subject, which, if you think them of sufficient importance, you will do me the honour of presenting to the Royal Society.

I am, &c.

ASTLEY COOPER.

Anatomists have endeavoured to ascertain, by experiments on quadrupeds, the loss of power which the organ of hearing would sustain by perforating the membrana tympani: dogs have been made the subject of these trials; but the results have been neither clear nor satisfactory, and they accord but little with the phænomena I am about to relate.

Mr. Cheselden had conceived the design of making the human organ itself the subject of direct experiment; and a con-

demned criminal was pardoned, on condition of his submitting to it; but, a popular outcry being raised, it was thought proper to relinquish the idea.

Though denied the aid of experiment, we are not without the means of obtaining knowledge upon such subjects; since the changes produced by disease, frequently furnish a clue which is equally satisfactory.

It often happens, that some parts of an organ are destroyed by disease, whilst others are left in their natural state; and hence, by the powers retained by such organ, after a partial destruction, we are enabled to judge of the functions performed by those parts, when the whole was in health.

Guided by this principle, I have made the human ear the subject of observation, and have endeavoured to ascertain the degree of loss it sustains in its powers by the want of the membrana tympani; a membrane which has been generally considered, from its situation in the meatus, and its connection with the adjacent parts by a beautiful and delicate structure, as essentially necessary to the sense of hearing; but which, as appears by the following observations, may be lost, with little prejudice to the functions of the organ.

Mr. P——, a medical student at St. Thomas's Hospital, of the age of twenty years, applied to me, in the winter of 1797, while he was attending a course of anatomical lectures, requesting my opinion upon the nature of a complaint in his ear, which had long rendered him slightly deaf.

Upon inquiring into the nature of the symptoms which had preceded, and of those which now accompanied the disease, he informed me, that he had been subject from his infancy to pains in the head, and was attacked, at the age of ten years, with an in-

flammation and suppuration in the left ear, which continued discharging matter for several weeks: in the space of about twelve months after the first attack, symptoms of a similar kind took place in the right ear, from which also matter issued for a considerable time. The discharge in each instance was thin, and extremely offensive to the smell; and, in the matter, bones or pieces of bones were observable. The immediate consequence of these attacks was a total deafness, which continued for three months; the hearing then began to return, and, in about ten months from the last attack, was restored to the state in which it at present remains.

Having thus described the disease and its symptoms, he gave me the following satisfactory proof of each membrana tympani being imperfect. Having filled his mouth with air, he closed the nostrils, and contracted his cheeks: the air, thus compressed, was heard to rush through the meatus auditorius, with a whistling noise, and the hair hanging from the temples became agitated by the current of air which issued from the ear. To determine this with greater precision, I called for a lighted candle, which was applied in turn to each ear, and the flame was agitated in a similar manner. Struck with the novelty of these phænomena, I wished to have many witnesses of them, and therefore requested him, at the conclusion of the lecture upon the organ of hearing, to exhibit them to his fellow students, with which request he was so obliging as to comply.

It was evident from these experiments, that the membrana tympani of each ear was incomplete, and that the air issued from the mouth, by the Eustachian tube, through an opening in that membrane, and escaped by the external meatus.

To determine the degree in which the membrana tympani

had been injured, I passed a probe into each ear, and found that the membrane on the left side was entirely destroyed; since the probe struck against the petrous portion of the temporal bone, at the interior part of the tympanum, not by passing through a small opening; for, after an attentive examination, the space usually occupied by the membrana tympani was found to be an aperture, without one trace of membrane remaining.

On the right side also, a probe could be passed into the cavity of the tympanum; but here, by conducting it along the sides of the meatus, some remains of the circumference of the membrane could be discovered, with a circular opening in its centre, about the fourth of an inch in diameter.

From such a destruction of this membrane, partial indeed in one ear, but complete in the other, it might be expected that a total annihilation of the powers of the organ would have followed: but the deafness was inconsiderable. This gentleman, if his attention were exerted, was capable, when in company, of hearing whatever was said in the usual tone of conversation; and it is worthy of remark, that he could hear with the left ear better than with the right, though in the left no traces of the membrana tympani could be perceived.

When attending the anatomical lectures also, he could hear, even at the most distant part of the theatre, every word that was delivered; though, to avoid the regular and constant exertion which it required, he preferred placing himself near the lecturer.

I found, however, that when a note was struck upon the piano forte, he could hear it only at two thirds of the distance at which I could hear it myself; and he informed me, that in a voyage he had made to the East Indies, while others, when

ships were hailed at sea, could catch words with accuracy, his organ of hearing received only an indistinct impression. But the most extraordinary circumstance in Mr. P—'s case is, that the ear was nicely susceptible of musical tones; *for he played well on the flute, and had frequently borne a part in a concert.* I speak this, not from his own authority only, but also from that of his father, who is an excellent judge of music, and plays well on the violin: he told me, that his son, besides playing on the flute, sung with much taste, and perfectly in tune.

The slight degree of deafness of which Mr. P— complained, was always greatly increased by his catching cold: an effect which seems to have arisen from the meatus being closed by an accumulation of the natural secretion of the ear; for it frequently happened to him, after he had been some time deaf from cold, that a large piece of hardened wax, during a fit of coughing, was forced from the ear, by the air rushing from the mouth through the Eustachian tube, and his hearing was instantly restored.

From bathing likewise he suffered considerable inconvenience, unless his ears were guarded against the water, by cotton being previously forced into the meatus. When this precaution was neglected, the water, as he plunged in, by rushing into the interior parts of the ears, occasioned violent pain, and brought on a deafness, which continued until the cause was removed, that is, until the water was discharged: but he had acquired the habit of removing it, by forcing air from the mouth through the ear.

In a healthy ear, when the meatus auditorius is stopped by the finger, or is otherwise closed, a noise similar to that of a distant roaring of the sea is produced: this arises from the air in the meatus being compressed upon the membrana tympani. In

the case here described, no such sensation was produced: for, in Mr. P's ear, the air, meeting with no impediment, could suffer no compression; since it found a passage, through the open membrane, to the mouth, by means of the Eustachian tube.

Mr. P— was liable to the sensation commonly called the *teeth being on edge*, in the same degree as it exists in others; and it was produced by similar acute sounds, as by the filing of a saw, the rubbing of silk, *&c.* Its occurring in him seems to disprove the idea which has been entertained of its cause; for it has been thought, that the close connection of the nerve called the corda tympani with the membrana tympani, exposed it to be affected by the motions of the malleus; and that, as it passes to nerves connected with the teeth, they would suffer from the vibratory state of the nerve, produced by the agitations of the membrane. But, in this case, as the membrane was entirely destroyed on that side on which the sensation was produced, some other explanation must be resorted to; and I see no reason why this effect should not be referred to that part of the auditory nerve which lines the labyrinth of the ear, which, being impressed by acute and disagreeable sounds, would convey the impression to the portio dura of the same nerve, and to the teeth with which that nerve is connected.

The external ear, though two distinct muscles are inserted into it, is capable, in its natural state, of little motion; however, when an organ becomes imperfect, every agent which can be employed to increase its powers is called into action; and, in the case here described, the external ear had acquired a distinct motion upward and backward, which was observable whenever Mr. P— listened to any thing which he did not distinctly hear. This power over the muscles was so great, that when desired

to raise the ear, or to draw it backwards, he was capable of moving it in either direction.

This case is not the only one of this description which has come under my observation; for another gentleman, Mr. A—, applied to me under a similar complaint, (but in one ear only,) proceeding from suppuration, and producing the same effects. This gentleman has the same power of forcing air through the imperfect ear; suffers equally from bathing, if the meatus auditorius be unprotected; and feels, even from exposure to a stream of cold air, very considerable pain. The only difference I could observe was, that in Mr. A's case, the defect of hearing in the diseased organ was somewhat greater than in the former; for though, when his sound ear was closed, he could hear what was said in a common tone of voice, yet he could not distinguish the notes of a piano forte at the same distance: a difference which might have in part arisen from the confused noise which is always produced by closing the sound ear; or because, as he heard well on one side, the imperfect ear had remained unemployed, and consequently had been enfeebled by disuse.

From these observations it seems evidently to follow, that the loss of the membrana tympani in both ears, far from producing total deafness, occasions only a slight diminution of the powers of hearing.

Anatomists who have destroyed this membrane in dogs, have asserted, that at first the effect on the sense of hearing was trivial; but that, after the lapse of a few months, a total deafness ensued. Baron HALLER also has said, that if the membrane of the tympanum be broken, the person becomes at first hard of hearing, and afterwards perfectly deaf. But, in these instances, the destruction must have extended further than the membrana

tympani; and the labyrinth must have suffered from the removal of the stapes, and from the consequent discharge of water contained in the cavities of the internal ear; for it has been very constantly observed, that when all the small bones of the ear have been discharged, a total deafness has ensued.

It is probable, that in instances in which the membrana tympani is destroyed, the functions of this membrane have been carried on by the membranes of the fenestra ovalis and fenestra rotunda: for, as they are placed over the water of the labyrinth, they will, when agitated by the impressions of sound, convey their vibrations to that fluid in a similar manner, though in somewhat an inferior degree, to those which are conveyed by means of the membrana tympani and the small bones which are attached to it; and thus, in the organ of hearing, each part is admirably adapted, not only to the purpose for which it is designed, but also as a provision against accident or disease; so that, whenever any particular part is destroyed, another is substituted for it, and the organ, from this deprivation, suffers but little injury in its functions.

It seems that the principal use of the membrana tympani is, to modify the impressions of sound, and to proportion them to the powers and expectation of the organ. Mr. P— had lost this power for a considerable period after the destruction of the membrane; but, in process of time, as the external ear acquired the additional motions I have described, sounds were rendered stronger or weaker by them. When, therefore, he was addressed in a whisper, the ear was seen immediately to move; but, when the tone of voice was louder, it then remained altogether motionless.

Some additional Remarks, on the Mode of Hearing in Cases where the Membrana Tympani has been destroyed. By EVERARD HOME, *Esq.*

After having communicated to this learned Society, the very curious facts contained in Mr. COOPER's paper, which prove that the organ of hearing is capable of receiving all the different impressions of sound, when the membrana tympani has been destroyed, it may not be improper to explain, from the observations contained in a former paper on this subject, in what manner this may take place.

It is there stated, that any vibrations communicated directly to the bones of the skull, are as accurately impressed upon the organ, as through the medium of the membrana tympani. The office of that membrane is therefore to afford an extended surface, capable of receiving impressions from the external air, and of communicating them to the small bones of the ear; which a membrane would be incapable of doing, unless it had a power of varying its tension, to adapt it to different vibrations.

In the above cases, in which this membrane, the malleus, and the incus, had been destroyed, it would appear that the stapes was acted upon by the air received into the cavity of the tympanum, and communicated the impressions immediately to the internal organ. This not happening for some months after the membrane was destroyed, probably arose from the inflammation of the tympanum confining the stapes, and rendering its vibrations imperfect.

That sounds can be communicated with accuracy by the bones of the skull, to the internal organ, when received from solid or liquid substances, has long been well understood.

That the membrana tympani is incapable of perfectly answering this purpose, when sounds are propagated through air, has been a generally received opinion; to refute which, was the object of my former paper. That in cases in which the membrana tympani has been destroyed, the air is capable of acting with sufficient force upon the stapes to communicate vibrations to it, and to produce on the internal organ the necessary effect for perfect hearing, is completely ascertained by Mr. COOPER'S observations.

IX. *Experiments and Observations on the Light which is spontaneously emitted, with some Degree of Permanency, from various Bodies.* By Nathaniel Hulme, *M. D. F. R. S. and A. S.*

Read February 13, 1800.

INTRODUCTION.

THE discoveries which have been made with respect to light, as it proceeds immediately from the sun, are many and important; but the observations on that species of light which is spontaneously emitted from various bodies, are not only few in number, but in general very imperfect. The author is therefore desirous of drawing the future attention of the philosopher more particularly to this subject, and of communicating his own experiments and observations upon it, to this learned Society.

By the spontaneous emission of this light, the author wishes to distinguish it from all kinds of artificial phosphorus; which, as he apprehends, differ essentially, in some of their properties, from that light of which he means to treat. And, by its adhesion to bodies with some degree of permanency, he distinguishes it from that transient sort of light which is observable in electricity, in meteors, and in other lucid emanations. The light which is the subject of this paper, he shall therefore beg leave to discriminate by the name of *spontaneous light*.

The substances from which such light is emitted, are principally the following.

Marine animals, both in a living state, and when deprived of life;

As instances of the first may be mentioned, the shell-fish called *Pholas*, the *Medusa phosphorea*, and various other *Mollusca*.

When deprived of life, marine fishes in general seem to abound with this kind of light. The honourable Mr. BOYLE commonly obtained light, for his use, from the whiting, as appears from many parts of his works: the author of these experiments and observations procured his fish light chiefly from the herring and the mackerel.

The flesh of quadrupeds has also been observed to emit light. Instances of this are mentioned by FABRICIUS *ab Aquapendente*; by T. BARTHOLIN; by Mr. BOYLE; and by Dr. BEALE; for which, see T. BARTHOLIN, *de Luce Animalium*, p. 183; BOYLE's Works, Vol. III. p. 304; Phil. Trans. Vol. XI. p. 599.

In the class of insects are many which emit light very copiously, particularly several species of *Fulgora* or Lantern-fly, and of *Lampyris* or Glow-worm; also the *Scolopendra electrica*; and a species of crab, called *Cancer fulgens*.

Rotten wood is well known to emit light spontaneously. Peat earth also has the same property. Of the effects of the latter, a remarkable instance is related in PLOT's Natural History of Staffordshire, p. 115.

The place where the following experiments were made, was a dark wine-vault, which, for distinction's sake, the author calls the *laboratory*. The heat of this laboratory varied, throughout the year, from about 40 degrees of temperature to 64°. The thermometer made use of was that of FAHRENHEIT.

The weight is always to be supposed that called Troy weight. The liquid measure employed, was that used for wine in this country: the ounce containing 8 drams Avoirdupois; and the pint, 16 ounces.

The water used in general for the experiments, was pure spring water, drawn up from under ground by means of a pump; and it was always employed cold, unless otherwise expressed.

SECTION I.

The Quantity of Light emitted by putrescent Animal Substances, is not in Proportion to the Degree of Putrefaction in such Substances, as is commonly supposed; but, on the contrary, the greater the Putrescence, the less is the Quantity of Light emitted.

EXPERIMENTS.

Exper. 1. Two very fresh herrings were bought in the morning, and hung up in the laboratory; on examining them in the evening, they were beginning to be luminous.

Exper. 2. Three herrings, which were quite fresh, after being scaled and gutted, were hung up by a string in the laboratory. The next evening they were become exceedingly luminous in every part, and much lucid matter had exuded, as it were, upon their surface, which was easily scraped off by the blunt edge of a knife; it also adhered to the fingers, or other parts of the body, when touched; but, as they grew more putrescent, the quantity of light diminished, and at last was extinguished.

Exper. 3. A single herring, that was perfectly sweet, was hung up in the laboratory. On the second night, it was covered with light; on the third, not so lucid; on the fourth, less so; and so on, in proportion to the degree of putrescence.

Exper. 4. Two herrings, somewhat stale, were hung up in the morning, and at 8 P. M. one of them was pretty luminous, but the other less so. On the next evening, the former

was but slightly luminous, and the latter was dark; on the succeeding evening, they were both dark.

Exper. 5. Two mackerels were brought from the market at 1 P. M. which, to the sight and smell, were perfectly sweet and good. Being then carried into the dark laboratory, and examined, the one was found to be a little luminous, and the other pretty much so, especially about its belly.

Exper. 6. A fine fresh mackerel, with a bright eye, was purchased about noon, and placed as usual in the laboratory, the temperature of which, at that time, was about 54°. At 11 P. M. this beautiful fish was luminous about the head and upper parts; and the inside of the mouth, which was wide open, shone with most brilliant light. The next evening, the whole body of the fish was very luminous: on the third night, it was less so; and on the fourth the light was nearly extinguished.

Exper. 7. In the forenoon, about ten o'clock, a couple of fine-looking mackerels were hung up in the laboratory, at the temperature of 56°, and at 10 P. M. they began to shine in various parts, the light seeming to proceed from within outwards. On the second night, they put on a luminous appearance all over their surface: on the third, the light was not so vivid; and on the fifth it was almost extinct.

N. B. In experiments of this kind, for the production of light, the fishes should always be gutted, the roes taken out, and the scales, if any, carefully removed. As the roes are likewise very productive of light, they should be preserved.

OBSERVATIONS.

Obs. 1. These experiments clearly prove, that light begins to be emitted by marine fishes, before any signs of putrefaction

appear: they likewise demonstrate, that as soon as a great degree of putrescence has taken place, the luminous property of the fishes is destroyed, and the light extinguished.

Obs. 2. In the instance of light proceeding spontaneously from animal flesh, recorded by AQUAPENDENTE, the flesh emitted light before any sensible putrescence had taken place, the meat being hung up in the larder for use. In that also mentioned by BARTHOLIN, in 1641, the flesh must have been fresh and sweet, for it was not intended to be dressed until the next day. Mr. BOYLE, in his report of light issuing from flesh, expressly says, that neither he, nor any of those who were about him, could perceive in it any offensive smell, whence to infer any putrefaction; the meat being judged very fresh, and well conditioned, and fit to be dressed. And, lastly, Dr. BEALE, in his account of a luminous neck of veal, says, that when it was dressed, on February the 27th, some of the neighbours, who saw it shining, were invited to eat of it, and all esteemed it as good as they had ever tasted; that a part of it was kept for February 28th and 29th, in which time it lost nothing of its sweetness.

Obs. 3. Whenever I wish to obtain a plentiful supply of light from fishes, for the purpose of experiments, I always endeavour to procure the freshest that can be had: long experience and frequent disappointments have taught me to adopt such a precaution.

SECTION II.

The Light here treated of is a constituent Principle of some Bodies, particularly of Marine Fishes, and may be separated from them, by a peculiar Process; may be retained, and rendered

permanent for some Time. It seems to be incorporated with their whole Substance, and to make a Part thereof, in the same Manner as any other constituent Principle.

EXPERIMENTS.

*The Flesh of Herring.**

Exper. 1. A fresh herring was split, or divided longitudinally, by a knife, into two parts. Then, about four drams of it, being cut across, were put into a solution, composed of two drams of Epsom salt or vitriolated magnesia, and two ounces of cold spring water drawn up by the pump. The liquid was contained in a wide-mouthed three-ounce phial, which was placed in the laboratory. Upon carefully examining the liquid, on the second evening after the process was begun, I could plainly perceive a lucid ring (for the phial was round) floating at the top of the liquid, the part below it being dark; but, on shaking the phial, the whole at once became beautifully luminous, and continued in that state. On the third evening, the light had again risen to the top; but the lucid ring appeared less vivid, and, on shaking the phial as before, the liquid was not so luminous as on the preceding night.

Exper. 2. The same experiment was repeated. On the second night, the liquid, being agitated, was very luminous; on the third, not so lucid; and on the fourth the light was extinguished.

Exper. 3. With sea salt or muriated natron half a dram, and two ounces of water. On the second night, the liquid, when agitated, was dark; on the third, lucid; on the fourth, very luminous; on the fifth, it began to lose light; on the sixth, it con-

* The quantity used in each experiment was about four drams.

tinued to decrease; and on the seventh it was quite gone. Neither the liquid, nor the herring, had contracted any putrid smell.

Exper. 4. With sea water two ounces. On the second night, dark; on the third, fourth, and fifth, luminous; on the sixth, nearly extinct; and on the seventh, totally. The piece of herring, when taken out and examined, was remarkably sweet.

*Roe of Herring.**

Exper. 5. With Epsom salt two drams, and water two ounces. On the second night, the liquid was pretty luminous; on the third and fourth, still luminous; and on the fifth its light was extinct.

Exper. 6. With GLAUBER's salt or vitriolated natron two drams, to two ounces of water. On the second night, when the phial was shaken, as usual in all these experiments, the liquid was pretty luminous; on the third, less so; and on the fourth the light was scarcely visible.

Exper. 7. With sea water two ounces. On the second night, dark; on the third, the liquid was moderately luminous; on the fourth and fifth, it had extracted much light; and on the seventh it was still shining. After this process, both the roe and the sea water remained perfectly sweet.

The Flesh of Mackerel.

Exper. 8. With Epsom salt two drams, and water two ounces. On the second night, the liquid was finely illuminated; on the third, a similar appearance; on the fourth, a diminution of light; on the fifth, it continued lucid in a small degree; and on the sixth the light was extinguished.

* The quantity used in each experiment was about four drams.

Roe of Mackerel.

Exper. 9. With Epsom salt two drams, and water two ounces. On the second night, the liquid, when agitated, was exceedingly bright; on the third, the same; and on the fourth and fifth, still lucid.

The Tadpole.

Exper. 10. It occurred to my mind, in the year 1797, to try what effect a saline menstruum would have upon the tadpole. Accordingly, I procured some tadpoles on the 10th of June, and put six of them into a solution of two drams of GLAUBER'S salt in two ounces of water. On the 11th, in the evening, the menstruum was dark; on the 12th, after shaking the phial, I was agreeably surprised to find it impregnated with light; on the 13th, the light was so abundant as to float on the top of the menstruum; on the 14th, the same phænomenon appeared; on the 15th and 16th, it was still present; on the 17th, the lucidness began to diminish; on the 18th, it was faint; and on the 19th it had vanished.

Exper. 11. On the 11th of June, six other tadpoles were dropped into a solution of one dram of common salt in three ounces of water. On the 12th and 13th, the menstruum was dark; on the 14th, it had extracted from the tadpoles a very beautiful bright light; on the 15th, the menstruum was exceedingly luminous; on the 16th and 17th, nearly the same: the light then gradually faded, so that on the 21st it was merely visible; and on the 22d it disappeared.

Exper. 12. On the 21st of June, the above two experiments were repeated; when the tadpoles remained in the menstruums

till the 27th, but no light was emitted. What was the cause of this failure in these two last experiments? Was it the ten days' increased growth of the animal, which was taken from the same pond, that made the difference?

Exper. 13. The above experiments were repeated, when the tadpole had just put on the state of a frog, but without producing any lucid appearance.

The Light is incorporated with the whole Substance of Marine Fishes.

Exper. 14. A fine fresh herring, being gutted, was divided longitudinally into two parts, both of which were hung up, by pieces of string, in the laboratory. On the 2d night, they were very lucid on the skinny side, but not on the fleshy or inward part; on the 3d, the fleshy or central parts of the fish, were thickly covered with a rich azure light; on the 4th, they continued exceedingly luminous; and on the 5th and 6th they were still lucid. It is surprising to think what a profusion of light was emitted from the interior substance of this single fish.

Exper. 15. A similar experiment was made with a mackerel, and with similar effects. These two experiments were frequently repeated.

Exper. 16. But the soft-roe, of both the herring and the mackerel, abounds more with light than even the flesh. When it is in its most luminous state, which generally happens about the 3d or 4th night, it will sometimes shine so very splendidly, as to appear like a complete body of light. It is remarkable that the hard-roe, in general, does not emit so much light as the soft-roe. When the roes were used, they were laid upon plates, and deposited in the laboratory.

OBSERVATIONS.

Obs. 1. The above experiments clearly prove, as I apprehend, that this light is a constituent principle of marine fishes: and that it is separated, by the menstruum employed on this occasion, in the same way that the principles of any other body are separated, by the menstruum fitted to decompose it. They likewise show, that it is not partially but wholly incorporated with every part of their substance, and makes a part thereof, in the same manner as any other constituent principle.

Obs. 2. Light is probably the first constituent principle that escapes, after the death of marine fishes. The experiments of the first Section teach us that it appears soon after death, even in fishes which, to the eye, seem quite fresh and sweet; or, at least, long before any sensible putrescence takes place. And we have seen that the flesh and roes, infused in the saline menstruums, continued to emit light for several days, without undergoing any apparent putrefactive change.

Obs. 3. The experiments likewise render it probable, that no offensive putrefaction takes place in the sea, after the death of such myriads of animals as must needs daily perish in the vast ocean, (quite contrary to what happens on land;) and that the flesh of marine fishes remains pretty sweet for some time, and may become wholesome food for many kinds of those which still remain alive. An eminent instance this, of the wisdom of the Creator, in the construction of the aqueous part of the world, which comprehends, by far, the greatest portion of the terraqueous globe, and is the most replete with animal life!

SECTION III.

Some Bodies or Substances have a Power of extinguishing spontaneous Light, when it is applied to them.

EXPERIMENTS.

The luminous matter proceeding from the herring and the mackerel, was quickly extinguished when mixed with the following substances: 1. Water alone. 2. Water impregnated with quicklime. 3. Water impregnated with carbonic acid gas. 4. Water impregnated with hepatic gas. 5. Fermented liquors. 6. Ardent spirits. 7. Mineral acids, both in a concentrated and diluted state. 8. Vegetable acids. 9. Fixed and volatile alkalis, when dissolved in water. 10. Neutral salts: *viz. saturated* solutions of Epsom salt, of common salt, and of sal ammoniac. 11. Infusions of chamomile flowers, of long pepper, and of camphor, made with boiling-hot water, but not used till quite cool. 12. Pure honey, if used alone.

SECTION IV.

Other Bodies or Substances have a Power of preserving spontaneous Light for some Time, when it is applied to them.

EXPERIMENTS.

Exper. 1. Some luminous matter scraped from the herring, was mixed with a solution of two drams of Epsom salt in two ounces of cold pump water: after shaking very well for some time the phial which contained them, the whole liquid became richly impregnated with light, and continued shining above twenty-four hours. This experiment was frequently repeated, and with the same effect.

Exper. 2. Two drams of GLAUBER's salt and two ounces of water being mixed with herring light, the solution was thereby quickly made very lucent, and remained so till the succeeding evening.

Exper. 3. Mackerel-light, being mixed with two drams of Rochelle salt or tartarized natron and two ounces of water, caused the fluid to be very luminous.

Exper. 4. Two drams of soda phosphorata and two ounces of water, mixed with herring-light, formed a very lucent fluid, which retained the light for a long time.

Exper. 5. Herring-light, with one dram of saltpetre or nitrated kali and two ounces of water, made the solution pretty luminous.

Exper. 6. Half a dram of common salt dissolved in two ounces of water, with the addition of mackerel-light, composed a very shining mixture, which retained its splendour for the space of a day or two. The same effect was produced by herring-light.

Exper. 7. Two ounces of sea water, being agitated with the light of a mackerel, soon obtained a brilliant illumination. The sea water preserved its luminousness for several days. The experiment was successfully repeated.

Exper. 8. Two drams of pure honey, that had not been clarified, or exposed to heat, were dissolved in two ounces of water; and, after the admission of some mackerel-light, and shaking the phial, the solution was fully impregnated with light, which was visible the next evening.

Exper. 9. Two drams of purified or refined sugar being dissolved in two ounces of water, and mixed with the shining matter of a herring, the fluid acquired a great degree of lucid-

ness. The same effect took place when the experiment was made with soft brown sugar.

N. B. It is almost needless to mention, that the degree of illumination in these liquids must depend upon the quantity of lucific matter applied; but, in general, as much as can be scraped off by the blunt point of a moderately-sized knife, at a few times, will be sufficient, being assisted by a strong agitation of the containing phial.

OBSERVATION.

These experiments enable us to take light and diffuse it through water, so as to render the whole liquid most brilliantly luminous, or, in other words, to impregnate water with light. By these means, the light is so extended in its surface, and combined in such a manner, as to become exceedingly convenient and useful for various other experiments.

SECTION V.

When spontaneous Light is extinguished by some Bodies or Substances, it is not lost, but may be again revived in its former Splendour, and that by the most simple Means.

EXPERIMENTS.

Exper. 1. On the 1st of June, 1795, the following experiments were made, to know what was the best proportion of Epsom salt to water, in order to produce the most luminous liquid. Some shining matter was taken from a mackerel, and mixed with a solution of seven drams of the salt in one ounce of water; and its light was immediately extinguished. The same effect ensued, but in a less degree, with a solution of six, and

one of five drams. In a solution of two drams, in the same quantity of water, the liquid was luminous; but much more so when only one dram of salt was used. Observing the extinction of light to take place, as above, in the more saturated solutions, while the diluted solutions were luminous, it occurred to me to endeavour to discover what became of the extinguished light, in the former case, and whether it might not be revived by dilution. For this purpose, I took the solution of seven drams of salt in one ounce of water, in which the lucid matter from a mackerel had been extinguished, and diluted it with six ounces of cold pump water; when, to my great astonishment, light in a moment burst out of darkness, and the whole liquid became beautifully luminous! This revived light remained above 48 hours, that is, as long as other light in general does, which has never been extinguished. Hence, it had lost nothing of its vivid luminous powers by its extinction.

Exper. 2. The last experiment was then reversed. A solution of one dram of Epsom salt in one ounce of water, was brilliantly illuminated with mackerel light. Then, six drams of the salt were put into this luminous liquid; and, after shaking the phial very well for a little time, to promote the solution of the salt, the light was totally extinguished. But the same light was again recovered, by the addition of six ounces of water.

In this manner, the light may be frequently extinguished, and as often revived. In one instance, the same light, by a repetition of this method, was made to undergo ten extinctions.

Exper. 3. A good quantity of herring-light, being mixed with a solution of four drams of common salt in two ounces of water, was immediately extinguished. Then, fourteen ounces of cold pump water were added thereto, and the whole liquid

was at once finely illuminated. On the next evening it appeared still very lucid; and likewise on the succeeding night.

Exper. 4. The experiment was reversed. Half a dram of the salt, being dissolved in two ounces of water, had herring-light mixed therewith, so as to be made very luminous. On the addition of two drams more of the salt, the lucidness was instantly destroyed: but the light was again recovered, by pouring eight ounces of cold water upon the extinguished luminous fluid. The revived light was very vivid the next evening.

Exper. 5. Two ounces of sea water were illuminated with mackerel-light, and then extinguished by adding two drams of common salt. The light was again restored, by diluting the solution with eight ounces of cold spring water.

N. B. If the illuminated liquid be uncommonly brilliant, it may sometimes require more salt to extinguish the light completely, than is here specified; in that case, the measure of water for dilution, must be always calculated in exact proportion to the weight of salt employed.

SECTION VI.

Spontaneous Light is rendered more vivid by Motion.

EXPERIMENTS.

Exper. 1. A quantity of illuminated liquid was poured into a broad vessel, which was placed in the laboratory. The next evening, on examination, it appeared to be quite dark. But a finger, or rod, being drawn through it, was followed by a luminous line.

Exper. 2. A phial, containing a pretty large portion of liquid impregnated with light, having been at rest a number of hours,

the liquid seemed to have lost its luminous quality, except a little glimmer floating at the top. It was then gently moved, and the light diffused itself gradually through the whole liquid: on agitation, the lucidness was much increased; and, the brisker the motion, the more vivid was the illumination.

SECTION VII.

Spontaneous Light is not accompanied with any Degree of sensible Heat, to be discovered by a Thermometer.

EXPERIMENTS.

Exper. 1. A luminous herring, and another that was quite fresh and not luminous, were placed for a considerable time in the same degree of temperature. A thermometer was then applied to each of them, but no difference of heat could be discovered.

Exper. 2. The soft roe of a herring, in an exceedingly lucid state, and a thermometer, were kept together for some time in the laboratory. The roe was then put upon the bulb of the thermometer, without affecting it.

Exper. 3. A mackerel, which shone with very brilliant light, was also put to the test of a thermometer, but the instrument remained stationary.

Exper. 4. The bulb of a thermometer was surrounded by many small pieces of shining wood, uncommonly luminous, which were kept in that situation for some time; but the light made no alteration upon the thermometer.

Exper. 5. Illuminated liquids, and spring water, being kept together in the laboratory, always preserved the same degree of temperature.

SECTION VIII.

The Effects of Cold on spontaneous Light.

EXPERIMENTS.

The Light of Fishes.

Exper. 1. Five small gallipots, containing three pieces of soft-roe of herring, and two of the herring itself, all very luminous, were placed in a frigorific mixture, composed of snow and sea-salt; and, in about an hour and a half, the light was quite extinct, and the bodies totally frozen. The gallipots were then removed into a vessel of cold water, that their contents might be gradually thawed; which being done, they all recovered their pristine luminous state. The pieces were afterwards observed to shine during three succeeding nights.

Exper. 2. A small phial, containing three or four drams of liquid impregnated with light, was placed in a frigorific mixture. As the liquid froze, its lucidness gradually diminished; and, when it was quite congealed, the light perfectly disappeared. The phial was then taken out, and put into cold water, at about 49° temperature, that the ice might be gradually liquefied; and, when that was accomplished, the whole fluid became as luminous as before.

The Light of shining Wood.

Exper. 3. A fragment of shining wood was put into a small wide-mouthed phial, which was plunged into a frigorific mixture. As the cold affected the wood, the light gradually faded,

and at last was totally imperceptible. The phial was then taken out, and placed in water at about 62°; by this change of temperature, the frozen wood gradually thawed, and then regained its former lustre.

The Light of Glow-worms.

Exper. 4. A small phial, containing a luminous dead glow-worm, was exposed to the cold of the frigorific mixture; as the coldness penetrated the phial, the light diminished, and at length was totally extinct. But, by placing the phial in water at about 62°, the glowing property of the insect soon returned. In this experiment, the glow-worm was evidently congealed; for it adhered to the side of the glass, and was covered with a hoar-frost. This experiment was frequently repeated, and with the same result.

OBSERVATION.

By these experiments we learn, that cold extinguishes spontaneous light in a temporary manner, but not durably, as the substances of the third Section do; because the light revived again in its full splendour, as soon as it was exposed to a moderate degree of temperature.

SECTION IX.

The Effects of Heat on spontaneous Light.

EXPERIMENTS.

The Light of Fishes.

Exper. 1. One side of a luminous herring was held before the fire, for a short space of time, but so as to receive its heat very strongly. It was then conveyed into the laboratory; when that side which had been exposed to the fire was found quite dark, but the other continued still luminous. The fish was preserved till the next evening, but the extinguished light did not re-appear.

Exper. 2. A whole herring, finely shining, was thrown into a quantity of boiling-hot water, and the light was immediately extinguished: after keeping it there for some time, it was taken out, but the light did not revive.

The Light of shining Wood.

Exper. 3. A piece of shining wood, its light being very faint, was put into tepid water at about 90 degrees of temperature, and it became in a short time much more lucid. Another piece, at 96°, was rendered beautifully luminous.

Exper. 4. A pretty thick piece of shining wood was put into a gallipot, and sunk under water by means of a weight, together with a thermometer, at the temperature of 64°. Boiling-hot water was then added by spoonfuls; and the light, at first, was rendered much more vivid, but soon after began to decrease, and was apparently extinct at about 110°. I say apparently, be-

cause on the next evening the light had somewhat revived; which shows, that the heat of 110° was not sufficient to extinguish totally all the light inherent in this piece of wood.

Exper. 5. Finding that 110 degrees of heat did not wholly extinguish the light of shining wood, a good many fragments, of different sizes, were then submitted to the power of boiling water, and detained therein for some time, in order that the heat might penetrate them thoroughly. The effect was, that the light became quickly extinct, and did not, as before, reappear on the following evening.

The Light of Glow-worms.

Exper. 6. A dead shining glow-worm was put upon two ounces of water, contained in a wide-mouthed phial, at the temperature of 58°. The phial was then sunk, about two or three inches deep, in boiling-hot water; and, as the heat communicated itself to the contents of the phial, the light of the glow-worm became much more vivid.

Exper. 7. Another lucid dead glow-worm was put into warm water, at 114°, to see if that degree of heat would extinguish the light; but, on the contrary, its glowing property was augmented. All the water was then poured off, yet the insect continued to shine for some length of time.

Exper. 8. The effect of that heat which is obtained from dry solid bodies by friction, was next tried upon the light of the glow-worm. Two living glow-worms were put into a one-ounce phial, with a glass stopple; and, though they were perfectly dark at the time, yet, if the phial was briskly rubbed with a silken or linen handkerchief, till it became pretty warm, it seldom failed to make them display their light very finely.

This experiment was very frequently repeated. It had the same illuminating effect upon the light of a dead glow-worm.

Exper. 9. The complete influence of 212 degrees of heat was now applied to the light of a glow-worm, by pouring upon one when dead, but in a luminous state, some boiling water. Its light was instantly extinguished thereby, and did not revive. The experiment was repeated, and with the same result.

Any of the saline Solutions mentioned in the fourth Section, being impregnated with luminous Matter, and left some Time at rest, are rendered more lucid by a moderate *Degree of Heat.*

Exper. 10. A quantity of illuminated solution was deposited in the laboratory. The next evening, when it was examined, it appeared in a manner quite dark; but, by putting the phial which contained it into hot water, the light revived, and was soon rendered exceedingly vivid.

Exper. 11. About a pint of solution impregnated with light, had become obscure, by time and rest, as is the nature of this mixture. Such a quantity of boiling-hot water was then added to it, as only to give it a small degree of warmth, and it quickly caused it to appear luminous.

Exper. 12. Illuminated liquid, to the quantity of four ounces, was placed in the laboratory until the next evening, when it had become almost dark. One spoonful of boiling-hot water being put into it, the light re-appeared; and, by means of two more, it was rendered considerably lucid.

Their Light is extinguished by a great *Degree of Heat.*

Exper. 13. Some boiling water being poured upon three or four ounces of illuminated liquid, in an earthen vessel, the light was immediately extinguished; and, though afterwards kept a

considerable time for inspection, and often agitated, to stir up the hidden light, yet no remains of any shining property could be perceived. This experiment was frequently repeated, and always with the same result.

Exper. 14. Four ounces of very luminous liquid, together with a thermometer, were put into a small earthen vessel, glazed white, the better to reflect light. Boiling-hot water was then added, by spoonfuls at a time, and by slow degrees. The first few spoonfuls made it considerably more lucid; and then, by adding more, the light began to fade, and at length was gradually extinguished. This effect took place, in one instance, when the liquid was heated to 96°; in another, to 98°; and in a third, to 100°. Hence, this species of light, when thus united with water, seems to begin to be extinguished at from 96 to 100 degrees of heat. This is a very elegant and pleasing method, of knowing how much heat is required to extinguish the light; because it measures it exactly, provided the hot water be added in small quantities, and by slow degrees, as above directed. To prevent the possibility of any light reviving after an experiment of this kind, would require a much greater heat than that of 100°. The intention of the present experiments was only to show, that all light may be apparently extinguished, at so low a degree of temperature as from 96° to 100°.

Exper. 15. A phial of an ounce and a half was filled with some very luminous liquid, but not corked. It was then suspended by a string, in a quart of boiling-hot water contained in a white earthen mug, and the light was wholly extinguished in about three or four minutes. After this, the phial was kept in the water some time longer, was then taken out to cool, and well shaken, but the light did not revive. It was examined the next day, and agitated again, but no luminous appearance could be

discovered; a proof that all the light had been totally extinguished by the power of heat.

If much Heat be applied to the Bottom of a Tube filled with illuminated Liquid, which has been some Time at rest, the Light will descend in luminous Streams, from the Top of the Tube to the Bottom, and be gradually extinguished.

Exper. 16. A glass cylindrical tube, closed at one end, being 9 inches long, with a bore of $1\frac{1}{10}$ inch, when used, was put into a gallipot $3\frac{1}{4}$ inches deep, and $3\frac{1}{2}$ wide, which held about 12 ounces of boiling water, and was placed in another larger vessel, to receive the overflowing water upon the immersion of the tube. The tube being filled over night with some very luminous liquid, was placed in the laboratory until the next evening. The light had then ascended plentifully to the top of the fluid, (the rest being dark,) and, taking the circular shape of the tube, formed a very lucid ring. The vessels with the boiling-hot water were then carried into the dark laboratory; and the tube being gently and carefully placed (without shaking) in the gallipot, the light was, generally in about half a minute, seen plainly to descend in streams from the top to the bottom, illuminating the whole fluid in its descent in a beautiful manner, and then was gradually extinguished. The extinction of the light began at the top of the tube, and ended at the bottom.

Exper. 17. The experiment was also made with a tube 19 inches high, $\frac{1}{2}$ an inch in bore, having several curvatures, and sealed hermetically at its lower end. Both the extremities were made straight for a few inches; the one to be immersed in the water, and the other to prevent the liquid running out. The luminous ring being formed as above mentioned, the tube was

put into the gallipot of boiling-hot water; and, in a short time, the light began to descend from the top, and came waving down, in a pleasing manner, to the bottom of the tube in the hot water, and then was by degrees extinguished. The whole length of the tube, including the curvatures, was 26 inches.

The most eligible solutions for this curious experiment, are those made with Epsom salt, GLAUBER'S salt, sea-salt, and sal ammoniac: if either of the two former be used, the proper proportion is, one dram of salt to each ounce of water; if either of the two latter, 15 grains to each ounce of water will be sufficient.

N. B. The experimentalist, before he views the descent of the light in the tube, should always remain in the dark for some little time, in order to get rid of all extraneous light adhering to the organs of vision, and to accommodate the eye to darkness.

SECTION X.

The Effects of the human Body, and of the animal Fluids, upon spontaneous Light.

The living Body.

Exper. 1. On touching the luminous matter of fishes, the light adhered to the fingers and different parts of the hands; remained very lucid for some little time, and then gradually disappeared. But the same kind of matter being applied to pieces of wood, stone, and the like, of the same temperature as the laboratory, continued luminous on these substances for many hours.

Exper. 2. A piece of red blotting-paper, about one inch square, and four times doubled, was finely illuminated by matter from a herring, and applied to the upper part of the inside of the thigh. After the expiration of 15 or 20 minutes, it was taken off; and, on examination, the light was quite extinguished. The experiment was repeated several times, and with the same effect. Another piece of the like paper was illuminated at the same time, and placed in the laboratory; where it retained its light above 48 hours.

Exper. 3. A piece of shining wood was placed upon the palm of the hand, and inclosed therein for some time; on inspection, it was found to be more lucid than before. Many trials of this kind were made, with the like success.

Exper. 4. A dead glow-worm, being but slightly luminous, was breathed upon several times; and its light increased both in magnitude and brightness. The experiment was frequently repeated, with the same result.

Animal Fluids.

Blood.

Exper. 5. A person having received a contusion, but otherwise in health, was bled. The next day, some herring-light was mixed with about two ounces of the crassamentum or red coagulated part of the blood, by stirring them well together with a knife: it caused it to be slightly luminous, but the light was not of long duration. Nearly the same result followed the mixture of lucid matter with the recent crassamentum of persons labouring under inflammatory diseases, as the pleurisy and rheumatism.

Exper. 6. But, when mixed with crassamentum that had been kept for some time, and become black and somewhat offensive to the smell, the light seemed to be more quickly extinguished.

Exper. 7. A singular phænomenon happened several times, on mixing fish-light with putrescent bloody serum. It would not incorporate, but was ejected in globules, like quicksilver when rubbed with any unctuous substance, and afterwards adhered to the side of the vessel in which the mixture was made, in the form of a lucid ring.

Exper. 8. The luminous matter of a herring was mixed with about two ounces of pure serum, from the healthy subject of the 5th experiment: it soon became finely illuminated, and retained its shining appearance for a long time, whenever it was stirred or agitated.

Exper. 9. The recent serum, drawn from patients afflicted with inflammatory complaints, was illuminated pretty much in the same manner as in the 8th experiment; and often retained light above 48 hours.

Urine.

Exper. 10. Mackerel-light being mixed, by strong agitation, with some fresh urine from a healthy person, a glimpse of light was retained at first, and then was gradually extinguished. But stale and pungent urine, being incorporated with luminous matter, had a still greater extinguishing effect.

Bile.

Exper. 11. Some bile, taken from a person who died of a suppression of urine, had herring-light mixed with it, which soon became extinct. Another trial was made, with a different bile, and with the same result.

Milk.

Exper. 12. Human milk not being easily obtained, some mackerel-light was incorporated, by agitation, with two ounces of fresh cow's milk, which was thereby rendered finely luminous, and continued shining above 24 hours. Fresh cream also retained some light; though it was not so visible as with milk, owing probably to its thickness. But, when either milk or cream turn sour, they contract a very extinguishing property. A quart of milk was kept five days, in a moderately cool place, in the month of June; by that time, it was changed into a mixture somewhat resembling curds and whey, that is, into a soft smooth coagulated part, and a very thin one, both which were acidulous. Some fine mackerel-light was mixed with two ounces of each of them, in separate phials, and they extinguished it immediately.

X. *Account of a Series of Experiments, undertaken with the View of decomposing the Muriatic Acid. By Mr.* William Henry. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. P. R. S.*

Read February 27, 1800.

Modern chemistry, notwithstanding its rapid advancement during the few last years, still presents to its cultivators several interesting objects, both of analytic and synthetic inquiry. Among the former, the decomposition of the muriatic and of certain other acids, holds a distinguished place; for our curiosity respecting the nature of these bodies, is strongly excited, by the influence which the discovery would have on the general doctrines of chemical science, as well as on the explanation of individual facts. The theory of the formation of acids, for example, one of the most important parts of the new system of chemistry, must be regarded as incomplete, and liable to subversion, till the individual acids now alluded to have been resolved into their constituent principles. To the best of my knowledge, however, we are not in possession of a single fact that gives the smallest insight into the constitution of the muriatic acid; and the attempts to effect its analysis, can only therefore be directed by the analogy of the decomposition of other bodies, which, from similarity of character, are arranged in the same class.

One of the first objects, in the analysis of a compound body,

should be its complete separation from all other substances, which, by their presence, may tend to introduce uncertainty into the results of the processes that are employed. But it is seldom that a simplicity so desirable can be attained in the objects of chemical research; for, agreeably to a known law of affinity, the last portions of any substance are separated with peculiar difficulty; the force of attraction appearing to increase, as we recede from the point of saturation. In a liquid state, the muriatic acid is a totally unfit subject for analytic experiment; for, in the strongest form under which it can be procured, it still contains a large proportion of water. This watery portion, besides the complexity which it introduces into the results of experiments, prevents any combustible substance that may be applied, from acting on the truly acid part; because that class of bodies, having less difficulty in attracting oxygen from the water than from the acid, will necessarily take it from the former source. The state of gas, therefore, is the only one in which the muriatic acid can become a proper object of analysis.

In the series of experiments on this gas, which I am now about to describe, I employed the electric fluid, as an agent much preferable to artificial heat. This mode of operating enables us to confine accurately the gases submitted to experiment; the phænomena that occur during the process, may be distinctly observed; and the comparison of the products, with the original gases, may be instituted with great exactness. The action of the electric fluid itself, as a decomponent, is extremely powerful; for it is capable of separating from each other, the constituent parts of water, of the nitric and sulphuric acids, of the volatile alkali, of nitrous gas, and of several other bodies, whose components are strongly united. I began, there-

fore, with examining attentively the effects of the electric fluid on the muriatic acid gas, without admixture.*

SECTION I.

On the Effects of Electricity on Muriatic Acid Gas.

When strong electrical shocks were passed through a portion of muriatic acid gas, confined in a glass tube over mercury, the following appearances took place. The bulk of the gas, after 20 or 30 shocks, was considerably diminished; and a white deposit appeared on the inner surface of the tube, which considerably obscured its transparency. In some instances, both the contraction and deposit were much more remarkable than in others. The gas which issued from muriate of soda, soon after the affusion of sulphuric acid, and while the charge was yet warm, exhibited these appearances in an eminent degree. Of this gas, 307 measures were reduced, by 20 shocks, to 227, or were contracted nearly one-fourth. Gas from the same materials, after they had continued working for some hours, was diminished, by similar treatment, only about a twelfth. These effects, therefore, it seemed probable, depended in some measure on the presence of moisture; and I accordingly found, that muriatic acid gas, after more than a week's exposure to muriate of lime, brought into contact with it immediately after cooling from a state of

* The gases submitted to the action of electricity, in the following experiments, were confined in straight glass tubes of various diameters, armed at the sealed end with a conductor of gold, or of platina, but generally of the latter metal. The shocks were as strong as could be given without breaking the tubes, which, notwithstanding every precaution, were often shattered by the force of the explosion. Each measure of gas is equal to the bulk occupied by a grain of mercury.

fusion, was scarcely diminished at all; and that the deposit, though it still occurred, was less copious in quantity. This deposit was not, like corrosive sublimate, soluble in water; but had every property of the less saturated salt, calomel.

The mercury by which the muriatic acid was confined, was therefore evidently oxidated; and, to the combination of a part of the gas with the oxide thus produced, the diminution of bulk was doubtless to be ascribed. But it was uncertain from whence this oxygen was derived. It might either result from the decomposition of the acid gas, or of the water chemically combined with it. The following experiments were therefore made, to determine this point.

Experiment 1. Through 1457 measures of muriatic acid gas, 300 electrical shocks were passed. There remained, after the admission of water, 100 measures of permanent gas, (or not quite 7 from each hundred of the original gas,) which, on trial, appeared to be purely hydrogenous.

Exper. 2. Of the gas, dried by muriate of lime, 176 measures received 120 shocks. The residue of hydrogenous gas amounted to 11 measures, or rather more than 6 *per cent.*

These experiments, and other similar ones, made on comparative portions of muriatic acid gas, in its recent state, and after exposure to muriate of lime, convinced me that it was impossible, by this method, wholly to deprive the muriatic gas of water. The recent gas, however, when electrified in smaller quantity than in experiment 1, gave a larger proportion of hydrogenous gas; which shews, that some portion of its moisture was removed, by exposure to muriate of lime. In order, if possible, to procure the gas perfectly dry, another mode of preparing it was resorted to. Alum and common salt were first well

calcined, separately, to expel their water of crystallization, and, being then mixed, were distilled together in an earthen retort. The gas proceeding from these materials, was received over dry mercury; but, though only the last portion that came over was reserved for experiment, it still, after the usual electrization, afforded a product of hydrogenous gas.

In the course of the preceding experiments, I observed that the diminution of the muriatic acid gas stopped always at a certain point, beyond which it could not be carried by continuing the shocks. Gas also, which had been thus treated, when transferred to another tube, and again electrified, did not exhibit any further deposit. It became interesting, therefore, to know, whether the production of hydrogenous gas had a similar limitation; because, the decision of this question would go far towards ascertaining its source. If the evolved hydrogenous gas arose from the decomposition of the acid, it might be expected to be produced, as long as any acid remained undecomposed. But, if water were the origin of this gas, it would cease to be evolved, when the whole of the water contained in the gas had been resolved into its constituent principles.

Experiments 3 and 4. Into two separate tubes, I passed known quantities of muriatic acid gas. Through the one portion, 200 discharges were taken; and through the other, 400. On comparing the quantities of hydrogenous gas produced, it proved to bear exactly the same proportion, in each tube, to the gas originally submitted to experiment. Hence it may be inferred, that the hydrogenous gas, evolved by electrifying the muriatic acid, has its origin, not from the acid, but from the water which is intimately attached to it. The agency of the electric fluid appears also, from the following experiments, to be exerted, not

only in disuniting the elements of water, but in promoting the union of the evolved oxygen with muriatic acid.

Exper. 5. A mixture of common air and muriatic acid gas, in the proportion of 143 of the former to 116 of the latter, was rapidly diminished by electrical shocks; 90 of which reduced the whole to 111.* The remainder consisted of muriatic acid and azotic gases, with a small proportion of oxygenous gas. The deposit formed on the tube was of the same kind as before, but much more abundant.

Exper. 6. The same appearances were occasioned, much more remarkably, by electrifying muriatic acid with oxygenous gas; and the contraction continued, till the mercury rose so as to touch the extremity of the platina conductor. At each explosion, a dense white cloud was seen in the tube, which soon settled on its inner surface, and was of exactly the same chemical composition as the one already described. Nitrous gas and muriatic gas, when electrified together, underwent a similar change.

In order to ascertain whether the mercury by which the gases were confined, in the above experiments, had any influence on their results, they were repeated in an instrument made, purposely for the occasion, by Mr. CUTHBERTSON, of London. It consisted of a glass tube, ground at each end, with the view of receiving two stoppers, each perforated with platina wire, which projected into the cavity of the tube. When the stoppers were in their places, the extremities of the wires were

* This experiment suggests an additional reason, to that already given, for the greater diminution of the first, than of the subsequent portions of muriatic acid gas; for the former may be presumed to have been much more adulterated than the latter, with the atmospherical air of the vessels.

at the distance of about half an inch; and, by properly disposing the apparatus, electrical shocks might be passed, through any gas or mixture of gases, with the contact only of glass and platina.

Exper. 7. In this tube I electrified the muriatic acid gas, and then admitted to it an infusion of litmus. The sudden destruction of its colour evinced the formation of oxygenated muriatic acid. Not the smallest deposit appeared on the tube.

Experiments 8 and 9. The same phænomenon took place, when an infusion of litmus was brought into contact with a mixture of common air and muriatic acid, and of oxygenous gas and muriatic acid, after electrization in this instrument; oxygenated muriatic acid being produced in both cases.

The above facts prove, that the combination of oxygen with muriatic acid, in these experiments, is not occasioned by a predisposing affinity in the mercury to combine with oxygenated muriatic acid; but that the electric fluid serves actually as an intermedium, in combining the muriatic acid with oxygen.

From the relation of these experiments it appears, that not the smallest progress had been made by them, towards the decomposition of the muriatic acid. I resolved, therefore, to attempt its analysis, in a similar manner, with the aid of combustible gases.

SECTION II.

Effects of electrifying the Muriatic Acid Gas with inflammable Substances.

In a memoir read before the Royal Society, and inserted in their Transactions for 1797, I have shewn, that when electrical

shocks are passed repeatedly through a confined portion of carbonated hydrogenous gas, the water held in solution by the gas, is decomposed by the carbon, which forms a constituent part of it; that carbonic acid is formed; and an addition made, of hydrogenous gas. Hence, the bulk of the carbonated hydrogen gas is considerably enlarged by this process; which shews, by its results, that the affinity of carbon for oxygen, is rendered much more powerful and efficient by the electric fluid. I have since found, that other oxygenated substances are decomposed, by electrifying them with carbonated hydrogen gas. Nitrous gas, for example, is speedily destroyed by this process, and carbonic acid and azotic gases are obtained.

Every attempt to decompose the muriatic acid, must be founded on the presumption that it is an oxygenated substance; and those bodies promise to be the most successful agents, that possess a strong affinity for oxygen. Now, of all known bodies, charcoal most strongly attracts oxygen; and I have, therefore, repeatedly attempted the destruction of this acid, by passing it over red-hot charcoal. But, in a series of experiments, which I made some time since, with this view, in conjunction with Mr. RUPP, we soon found reason to be dissatisfied with the difficulty and uncertainty of this process. An immense production of hydrogenous gas took place; but it was not easy to determine whether it had its origin from real acid, or from water. Our experiments, however, though insufficient to furnish decisive proof, induced us to believe that it had the latter origin.

It next occurred to me, that the comparative affinities of the muriatic radical, whatever it may be, and of charcoal, for oxygen, would be elegantly and satisfactorily ascertained, by electrifying together the carbonated hydrogen and muriatic gases. If the

muriatic acid be capable of decomposition by carbon, it might be expected to be destroyed by this process; and the exact quantity of acid decomposed, and the nature and quantity of the products, would thus be easily determined. I electrified, therefore, the muriatic acid and carbonated hydrogen gases, with the most scrupulous attention to the phænomena and results. That the electric fluid might not be misapplied, in decomposing the water of the carbonated hydrogen gas, it was kept more than a week, before use, over quick lime, introduced to it while yet hot.

Exper. 10. Of this carbonated hydrogenous gas, 186 measures were expanded, by 130 shocks, to 211; that is, the gas was increased about $\frac{1}{8}$ its bulk.

Exper. 11. Of the same gas, 84 measures were mixed with 116 of muriatic acid gas, dried by muriate of lime. By 120 shocks, the mixture was a little dilated. After the admission of a drop or two of water, there remained 91 measures; *i. e.* the addition of permanent gas was 7 measures, or about as much as might have been expected from the muriatic gas alone.

Exper. 12. Eighty-three measures of dry carbonated hydrogenous gas, with 89 of muriatic acid gas, received 200 shocks. The permanent residue, after the admission of water, was 101 measures: the addition, therefore, amounted to 18. Of the added 18, 6 may be accounted for by the decomposition of the water of the muriatic gas, and 10 by that of the carbonated hydrogenous gas. There remain, therefore, only 2 measures that can be supposed to be produced from the muriatic acid gas; a quantity too small to afford grounds for supposing them to arise from decomposed acid.

Exper. 13. Dry carbonated hydrogenous gas 132 measures,
mixed with dry muriatic gas - 108
240
by 200 shocks, expanded to - 268

Part of this gas was then transferred to another tube, and the proportion of permanent gas ascertained. Through the remainder, 150 additional shocks were passed, before the amount of the gas thus evolved was determined. In both, it bore exactly the same proportion to the original gas; which shews, that by continuing the electrization, no further effects were produced.

A great variety of similar experiments convinced me, that by electrifying together the carbonated hydrogenous and muriatic gases, not the smallest progress was made towards the decomposition of the latter. All that was thus effected, consisted in the decomposition of the water of the two gases, by the carbon of the combustible gas; and, when this was completely accomplished, no further effect ensued from continuing the electrization. The generation of carbonic acid was proved, by the following experiment.

Exper. 14. To a mixture of carbonated hydrogen and muriatic gases, after having received above 100 shocks, a drop of water was admitted, which absorbed the muriatic acid. The liquid was then taken up by blotting-paper; and the residuary gas, being transferred into another tube, was brought into contact with a solution of pure barytic earth. The precipitation of this solution, evinced the presence of carbonic acid.

It was desirable, however, that the effects should be ascertained, of electrifying together pure muriatic acid and pure carbonated hydrogenous gas, both perfectly free from water. Now,

from the experiments related in the first Section, it appears highly probable, that a complete purification from moisture is produced, in both gases, by the action of the electric fluid; all the water they before contained being thus decomposed. In the following experiments, therefore, the two gases were separately electrified, before they were submitted to this process conjointly.

Exper. 15. To a portion of muriatic acid, diminished by the action of electricity from 144 to 121 measures, 27 measures of carbonated hydrogenous gas, expanded as far as possible, were added, and 200 shocks passed through the mixture. The addition of permanent gas amounted to 14 measures; 10 of which may be traced to the muriatic acid, and were evolved by its separate electrization. The remaining 4 measures, which remain to be accounted for, are too small a quantity to be ascribed to the decomposition of the acid.

Exper. 16. To a quantity of carbonated hydrogenous gas, which had received 400 shocks, and occupied the space of 212 measures, I added 232 of muriatic acid, through which 200 shocks had previously been passed. The electrization of the mixture was next continued, till 800 discharges had taken place. On examining the mixture of gases, during this operation, no change whatever took place; and, after its close, no more muriatic acid had disappeared, than would have been deficient after the first electrization; nor was there any further production of permanent gas.

Exper. 17. The same result was obtained, by electrifying together 280 measures of carbonated hydrogenous gas, previously expanded by 600 shocks, and 114 of muriatic acid, after 400 shocks. The additional discharge, through this mixture,

of 1000 shocks, did not evince the smallest progress towards the decomposition of the muriatic acid.

Exper. 18. In the naturally moist state of these gases, it follows, from the 14th experiment, that carbonic acid is produced by electrifying them in conjunction. It appeared to me of some importance to ascertain whether, after a previous decomposition of their moisture, carbonic acid would continue to be generated. But the electrified carbonated hydrogenous gas itself contains carbonic acid, which, unless removed, would render the result of the experiment undecisive. This was accomplished by passing up, to a portion of electrified gas, a bubble or two of dry ammoniacal gas, which, uniting with the carbonic acid, would condense any portion of it that might be present. The remainder was transferred into another tube; and, to this carbonated hydrogenous gas, perfectly deprived both of moisture and carbonic acid, muriatic acid gas, previously electrified, was added, and electrical shocks were passed through the mixture. A drop of water was then admitted; and the residuary gas, after having been dried, was transferred into another tube. On passing up barytic water, not the smallest trace of carbonic acid could be discovered.

From the preceding experiments, the following conclusions may be deduced.

1. The muriatic acid gas, in the driest state in which it can be procured, still contains a portion of water. From a calculation founded on the experiments described in the first section, the grounds of which are too obvious to require being stated, it follows, that 100 cubical inches of muriatic gas, after exposure to muriate of lime, still hold in combination 1.4 grain of water.

2. When electrical shocks are passed through this gas, the

watery portion is decomposed. The hydrogen of the water, uniting with the electric matter, constitutes hydrogenous gas, and the oxygen unites with the muriatic acid; which last, acting on the mercury, composes muriate of mercury.

3. The electric fluid serves as an intermedium, in combining oxygen with muriatic acid.

4. The really acid portion of muriatic gas does not sustain any decomposition by the action of electricity.

5. When electric shocks are passed through a mixture of carbonated hydrogen and muriatic acid gases, the water held in solution by these gases, is decomposed by the carbon of the compound inflammable gas; and carbonic acid and hydrogenous gases are the result.

6. When all the water of the two gases has been decomposed, no effect ensues from continuing the electrization; or, if the water of each gas has been previously destroyed, by electrifying them separately, no further effect ensues from electrifying them conjointly.

7. Since therefore carbon, though placed under the most favourable circumstances for abstracting from the muriatic acid, and combining with, its oxygen, evinces no such tendency, it may be inferred, that if the muriatic acid be an oxygenated substance, its radical has a stronger affinity for oxygen than charcoal possesses.

Though the first impressions excited in my mind by the total failure of the above experiments, in accomplishing one of the greatest objects of modern chemistry, have induced me for some time to withhold them from the society, I am satisfied by reflection, that this communication is not without expediency. The means employed in attempting the analysis of the muriatic acid,

were such as, after mature deliberation, appeared to me most to promise success; and the experiments were attended with a degree of labour, which can only be estimated by those who have been engaged in similar pursuits; not one third of those which were really made having been described, in the foregoing account of them. It may spare therefore to others, a fruitless application of time and trouble, to be made acquainted with what I have done; and the collateral facts, which have presented themselves in the inquiry, are perhaps not without curiosity or value.

From the result of these experiments, I apprehend, all hope must be relinquished, of effecting the decomposition of the muriatic acid, in the way of single elective affinity. They furnish also a strong probability, that the basis of the muriatic acid is some unknown body; for, no combustible substance with which we are acquainted, can retain oxygen, when submitted, in contact with charcoal, to the action of electricity, or of a high temperature. The analysis of this acid must, in future, be attempted with the aid of complicated affinities. Thus, in the masterly experiment of Mr. TENNANT, phosphorus, which attracts oxygen less strongly than charcoal, by the intermediation of lime decomposes the carbonic acid. Yet, led by the analogy of this fact, its discoverer found that a similar artifice did not succeed in decomposing the muriatic acid. "As vital "air," he observes, "is attracted by a compound of phosphorus "and calcareous earth, more powerfully than by charcoal, I was "desirous of trying their efficacy upon those acids which may "from analogy be supposed to contain vital air, but which are "not affected by the application of charcoal. With this intention, "I made phosphorus pass through a compound of marine acid

"and calcareous earth, and also of fluor acid and calcareous "earth, but without producing in either of them any alteration. "Since the strong attraction which these acids have for calca- "reous earth tends to prevent their decomposition, it might be "thought, that in this manner they were not more disposed to "part with vital air than by the attraction of charcoal: but "this, however, does not appear to be the fact. I have found, "that phosphorus cannot be obtained by passing marine acid "through a compound of bones and charcoal when red-hot. "The attraction, therefore, of phosphorus and lime for vital "air, exceeds the attraction of charcoal, by a greater force than "that arising from the attraction of marine acid for lime."*

By means similar to those employed in attempting the analysis of the muriatic acid, I tried to effect that of the fluoric acid. When electrified alone, in a glass tube coated internally with wax, it sustained a diminution of bulk, and there remained a portion of hydrogenous gas. But, neither in this mode, nor by submitting it, mixed with carbonated hydrogenous gas, to the action of electricity, was any progress made towards its analysis. These experiments, however, render it probable, that the fluoric acid, like the muriatic, is susceptible of still farther oxygenation, in which state it becomes capable of acting on mercury. The carbonic acid, on the contrary, appears not to admit of different degrees of oxygenation. When the electric shock has been repeatedly passed through a portion of this acid gas, its bulk is enlarged, and a permanent gas is produced, which is evidently

* Phil. Trans. Vol. LXXXI. p. 194.

a mixture of oxygenous and hydrogenous gases; for, when an electrical spark is passed through the gas that remains after the absorption of the carbonic acid by caustic alkali, it immediately explodes. These results even take place on electrifying carbonic acid from marble, previously calcined in a low red heat, to expel its water, and then distilled in an earthen retort.*

* Messieurs LANDRIANI and VAN MARUM (*Annales de Chimie*, Tom. II. p. 270.) obtained only hydrogenous gas, by electrifying the carbonic acid gas. But the conductor of their apparatus was an iron one; which metal would combine with the oxygen of the water, and prevent it from appearing in a gaseous state. In my experiments, the conductors were of platina.

XI. *On a new fulminating Mercury.* By Edward Howard, *Esq.* *F. R. S.*

Read March 13, 1800.

SECTION I.

THE mercurial preparations which fulminate, when mixed with sulphur, and gradually exposed to a gentle heat, are well known to chemists: they were discovered, and have been fully described, by Mr. BAYEN.*

MM. BRUGNATELLI and VAN MONS have likewise produced fulminations by concussion, as well with nitrate of mercury and phosphorus, as with phosphorus and most other nitrates.† Cinnabar likewise is amongst the substances which, according to MM. FOURCROY and VAUQUELIN, detonate by concussion with oxymuriate of potash.‡

Mr. AMEILON had, according to Mr. BERTHOLLET, observed, that the precipitate obtained from nitrate of mercury by oxalic acid, fuses with a hissing noise.§

* *Opuscules Chimique de Bayen*, Tom. I. p. 346, and note in p. 344.

† *Annales de Chimie*, Tom. XXVII. p. 74 and 79.

‡ Ibid. Tom. XXI. p. 238.

§ This fact has been misrepresented, in the introduction to a work intitled The chemical Principles of the metallic Arts, by W. RICHARDSON, surgeon, F. A. S. Sc. (page lvii.) The author, speaking of the acid of sorrel, says, "KLAPROTH of Berlin "precipitated a nitrous solution of mercury with acid of wood-sorrel, neutralized with

SECTION II.

But mercury, and most if not all its oxides, may, by treatment with nitric acid and alcohol, be converted into a whitish crystallized powder, possessing all the inflammable properties of gunpowder, as well as many peculiar to itself.

I was led to this discovery, by a late assertion, that hydrogen is the basis of the muriatic acid: it induced me to attempt to combine different substances with hydrogen and oxygen. With this view, I mixed such substances with alcohol and nitric acid, as I thought might (by predisposing affinity) favour, as well as attract, an acid combination, of the hydrogen of the one, and the oxygen of the other. The pure red oxide of mercury appeared not unfit for this purpose; it was therefore intermixed with alcohol, and upon both, nitric acid was affused. The acid did not act upon the alcohol so immediately as when these fluids are alone mixed together, but first gradually dissolved the oxide: however, after some minutes had elapsed, a smell of ether was perceptible, and a white dense smoke, much

" vegetable alkali. The white precipitate, well washed and dried, produced a fulmi-
" nating noise, not inferior to that of fulminating gold. Acid of sugar, perfectly neu-
" tralized by vegetable alkali, produced the same precipitate, which, on exposure to
" heat, exhibited the same fulminating power." I must confess, I have not been able to
" produce any such fulmination." Mr. RICHARDSON has moreover given this supposed discovery to Mr. KLAPROTH; whereas, Mr. BERTHOLLET, when quoting the fact to which I suppose Mr. RICHARDSON intended to allude, observes, " Qu'on avoit déjà
" donné le nom d'argent fulminant au précipité du nitrate d'argent par l'acide oxa-
" lique, dans lequel M. KLAPROTH avoit découvert la propriété de fuser avec vivacité
" lorsqu'on l'expose à la chaleur. M. AMEILON avoit aussi, depuis longtems, fait
" connoître que l'acide oxalique communiquoit cette propriété au mercure, quoique
" moins fortement qu'à l'argent; mais cet effet (he continues) est fort éloigné de
" celui qu'on désigne par la fulmination." *Annales de Chimie*, Tom. I. p. 57.

resembling that from the *liquor fumans* of LIBAVIUS, was emitted with ebullition. The mixture then threw down a dark-coloured precipitate, which by degrees became nearly white. This precipitate I separated by filtration; and, observing it to be crystallized in small acicular crystals, of a saline taste, and also finding a part of the mercury volatilized in the white fumes, I must acknowledge I was not altogether without hopes that muriatic acid had been formed, and united to the mercurial oxide. I therefore, for obvious reasons, poured sulphuric acid upon the dried crystalline mass, when a violent effervescence ensued, and, to my great astonishment, an explosion took place.

The singularity of this explosion induced me to repeat the process several times; and, finding that I always obtained the same kind of powder, I prepared a quantity of it, and was led to make the series of experiments which I shall have the honour to relate in this paper.

SECTION III.

I first attempted to make the mercurial powder fulminate by concussion; and for that purpose laid about a grain of it upon a cold anvil, and struck it with a hammer, likewise cold: it detonated slightly, not being, as I suppose, struck with a flat blow; for, upon using 3 or 4 grains, a very stunning disagreeable noise was produced, and the faces both of the hammer and the anvil were much indented.

Half a grain or a grain, if quite dry, is as much as ought to be used on such an occasion.

The shock of an electrical battery, sent through 5 or 6 grains of the powder, produces a very similar effect: it seems indeed,

that a strong electrical shock, generally acts on fulminating substances like the blow of a hammer. Messrs. FOURCROY and VAUQUELIN found this to be the case with all their mixtures of oxymuriate of potash.*

To ascertain at what temperature the mercurial powder explodes, 2 or 3 grains of it were floated on oil, in a capsule of leaf tin; the bulb of a FAHRENHEIT's thermometer was made just to touch the surface of the oil, which was then gradually heated till the powder exploded, as the mercury of the thermometer reached the 368th degree.

SECTION IV.

Desirous of comparing the strength of the mercurial compound with that of gunpowder, I made the following experiment, in the presence of my friend Mr. ABERNETHY.

Finding that the powder could be fired by flint and steel, without a disagreeable noise, a common gunpowder proof, capable of containing eleven grains of fine gunpowder, was filled with it, and fired in the usual way: the report was sharp, but not loud. The person who held the instrument in his hand felt no recoil; but the explosion laid open the upper part of the barrel, nearly from the touch-hole to the muzzle, and struck off the hand of the register, the surface of which was evenly indented, to the depth of 0,1 of an inch, as if it had received the impression of a punch.

The instrument used in this experiment being familiarly known, it is therefore scarcely necessary to describe it; suffice it to say, that it was of brass, mounted with a spring register,

* *Annales de Chimie*, Tom. XXI. p. 239.

the moveable hand of which closed up the muzzle, to receive and graduate the violence of the explosion. The barrel was half an inch in caliber, and nearly half an inch thick, except where a spring of the lock impaired half its thickness.

SECTION V.

A gun belonging to Mr. Keir, an ingenious artist of Camden-town, was next charged with 17 grains of the mercurial powder, and a leaden bullet. A block of wood was placed at about eight yards from the muzzle, to receive the ball, and the gun was fired by a fuse. No recoil seemed to have taken place; as the barrel was not moved from its position, although it was in no ways confined. The report was feeble: the bullet, Mr. Keir conceived, from the impression made upon the wood, had been projected with about half the force it would have been by an ordinary charge, or 68 grains, of the best gunpowder. We therefore recharged the gun with 34 grains of the mercurial powder; and, as the great strength of the piece removed any apprehension of danger, Mr. Keir fired it from his shoulder, aiming at the same block of wood. The report was like the first in section IV. sharp, but not louder than might have been expected from a charge of gunpowder. Fortunately, Mr. Keir was not hurt, but the gun was burst in an extraordinary manner. The breech was what is called a patent one, of the best forged iron, consisting of a chamber 0,4 of an inch thick all round, and 0,4 of an inch in caliber; it was torn open and flawed in many directions, and the gold touch-hole driven out. The barrel, into which the breech was screwed, was 0,5 of an inch thick; it was split by a single crack three inches long, but this

did not appear to me to be the immediate effect of the explosion. I think the screw of the breech, being suddenly enlarged, acted as a wedge upon the barrel. The ball missed the block of wood, and struck against a wall, which had already been the receptacle of so many bullets, that we could not satisfy ourselves about the impression made by this last.

SECTION VI.

As it was pretty plain that no gun could confine a quantity of the mercurial powder sufficient to project a bullet, with a greater force than an ordinary charge of gunpowder, I determined to try its comparative strength in another way.

I procured two blocks of wood, very nearly of the same size and strength, and bored them with the same instrument to the same depth. The one was charged with half an ounce of the best Dartford gunpowder, and the other with half an ounce of the mercurial powder; both were alike buried in sand, and fired by a train communicating with the powders by a small touch-hole. The block containing the gunpowder was simply split into three pieces: that charged with the mercurial powder was burst in every direction, and the parts immediately contiguous to the powder were absolutely pounded, yet the whole hung together, whereas the block split by the gunpowder had its parts fairly separated. The sand surrounding the gunpowder was undoubtedly most disturbed: in short, the mercurial powder appeared to have acted with the greatest energy, but only within certain limits.

SECTION VII.

The effects of the mercurial powder, in the last experiments, made me believe that it might be confined, during its explosion, in the centre of a hollow glass globe. Having therefore provided such a vessel, 7 inches in diameter, and nearly half an inch thick, mounted with brass caps, and a stop cock, (see Plate VIII.) I placed 10 grains of the mercurial powder on very thin paper, laid an iron wire 149th of an inch thick across the paper, through the midst of the powder, and, closing the paper, tied it fast at both extremities, with silk, to the wire. As the inclosed powder was now attached to the middle of the wire, each end of which was connected with the brass caps, the packet of powder became, by this disposition, fixed in the centre of the globe. Such a charge of an electrical battery was then sent along the wire, as a preliminary experiment* had shewn me would, by making the wire red-hot, inflame the powder. The glass globe withstood the explosion, and of course retained whatever gases were generated: its interior was thinly coated with quicksilver in a very divided state. A bent glass tube was now screwed to the stop-cock of the brass cap, which being introduced under a glass jar standing in the mercurial bath, the stop-cock was opened. Three cubical inches of air rushed out, and a fourth was set at liberty when the apparatus was removed to the water-tub. The explosion being repeated, and the air all received over water, the quantity did not vary. To avoid an error from change of temperature, the glass globe was, both before and after the explosion, immersed in water of the same

* With Mr. CUTHBERTSON's electrometer.

temperature. It appears therefore, that the ten grains of powder, produced four cubical inches only of air.

To continue the comparison between the mercurial powder and gunpowder, 10 grains of the best Dartford gunpowder were in a similar manner set fire to in the glass globe: it remained entire. The whole of the powder did not explode, for some complete grains were to be observed adhering to the interior surface of the glass. Little need be said of the nature of the gases generated during the combustion of gunpowder: they must have been, carbonic acid gas, sulphureous acid gas, nitrogen gas, and (according to LAVOISIER*) perhaps hydrogen gas. As to the quantity of these gases, it is obvious that it could not be ascertained; because the two first were, at least in part, speedily absorbed by the alkali of the nitre, left pure after the decomposition of its nitric acid.

SECTION VIII.

From the experiments related in the 4th and 5th sections, in which the gunpowder proof and the gun were burst, it might be inferred, that the astonishing force of the mercurial powder is to be attributed to the rapidity of its combustion; and, a train of several inches in length being consumed in a single flash, it is evident that its combustion must be rapid. From the experiments of the 6th and 7th sections, it is sufficiently plain that this force is restrained to a narrow limit; both because the block of wood charged with the mercurial powder was more shattered

* See LAVOISIER, *Traité elementaire*, p. 527.

than that charged with the gunpowder, whilst the sand surrounding it was least disturbed; and likewise because the glass globe withstood the explosion of 10 grains of the powder fixed in its centre: a charge I have twice found sufficient to destroy old pistol barrels, which were not injured by being fired when full of the best gunpowder. It also appears, from the last experiment, that 10 grains of the powder, produced by ignition four cubical inches only of air; and it is not to be supposed that the generation, however rapid, of four cubical inches of air, will alone account for the described force; neither can it be accounted for by the formation of a little water, which, as will hereafter be shewn, happens at the same moment: the quantity formed from 10 grains must be so trifling, that I cannot ascribe much force to the expansion of its vapour. The sudden vaporization of a part of the mercury, seems to me a principal cause of this immense yet limited force; because its limitation may then be explained, as it is well known that mercury easily parts with caloric, and requires a temperature of 600 degrees of FAHRENHEIT, to be maintained in the vaporous state. That the mercury is really converted into vapour, by ignition of the powder, may be inferred from the thin coat of divided quicksilver, which, after the explosion in the glass globe, covered its interior surface; and likewise from the quicksilver with which a tallow candle, or a piece of gold, may be evenly coated, by being held at a small distance from the inflamed powder. These facts certainly render it more than probable, although they do not demonstrate, that the mercury is volatilized; because it is not unlikely that many mercurial particles are mechanically impelled against the surface of the glass, the gold, and the tallow.

As to the force of dilated mercury, Mr. BAUME' relates a remarkable instance of it, as follows.

"Un alchymiste se présenta à Mr. GEOFFROY, et l'assura "qu'il avoit trouvé le moyen de fixer le mercure par une opéra- "tion fort simple. Il fit construire six boîtes rondes en fer fort "épais, qui entroient les unes dans les autres; la derniere étoit "assujettie par deux cercles de fer qui se croisoient en angles "droits. On avoit mis quelques livres de mercure dans la capa- "cité de la première: on mit cet appareil dans un fourneau "assez rempli de charbon pour faire rougir à blanc les boîtes "de fer; mais, lorsque la chaleur eut pénétré suffisamment le "mercure, les boîtes creverent, avec une telle explosion qu'il se "fit un bruit épouvantable: des morceaux de boîtes furent "lancés avec tant de rapidité, qu'il y en eut qui passerent au "travers de deux planchers; d'autres firent sur la muraille des "effets semblables à ceux des éclats de bombes."*

Had the alchemist proposed to fix water by the same apparatus, the nest of boxes must, I suppose, have likewise been ruptured; yet it does not follow that the explosion would have been so tremendous: indeed it is probable that it would not, for if (as Mr. KIRWAN remarked to me) substances which have the greatest specific gravity, have likewise the greatest attraction of cohesion, the supposition that the vapour of mercury exceeds in expansive force the vapour of water, would agree with a position of Sir ISAAC NEWTON, *that those particles recede from one another with the greatest force, and are most difficultly brought together, which upon contact cohere most strongly.*†

* *Chymie expérimentale et raisonnée*, Tom. II. p. 393. Paris, 8°, 1773.

† NEWTON'S Optics, p. 372, 4th Ed. Lond. 1730.

SECTION IX.

Before I attempt to investigate the constituent principles of this powder, it will be proper to describe the process and manipulations which, from frequent trials, seem to me best calculated to produce it.

100 grains, or a greater proportional quantity, of quicksilver (not exceeding 500 grains*) are to be dissolved, with heat, in a measured ounce and a half of nitric acid.† This solution being poured cold *upon* two measured ounces of alcohol,‡ *previously* introduced into any convenient glass vessel, a moderate heat is to be applied until an effervescence is excited. A white fume then begins to undulate on the surface of the liquor; and the powder will be gradually precipitated, upon the cessation of action and re-action. The precipitate is to be immediately collected on a filter, well washed with distilled water, and carefully dried in a heat not much exceeding that of a water bath. The immediate edulcoration of the powder is material, because it is liable to the re-action of the nitric acid; and, whilst any of that acid adheres to it, it is very subject to the influence of light. Let it also be cautiously remembered, that the mercurial solution is to be poured *upon* the alcohol.

I have recommended quicksilver to be used in preference to an oxide, because it seems to answer equally, and is less expen-

* The reason of this limitation is not on account of any danger attending the process; but because the quantities of nitric acid and alcohol required for more than 500 grains, would excite a degree of heat detrimental to the preparation.

† Of the specific gravity of about 1,3.

‡ Of the specific gravity of about ,849.

sive; otherwise, not only the pure red oxide, but the red nitrous oxide, and turpeth, may be substituted; neither does it seem essential to attend to the precise specific gravity of the acid, or the alcohol. The rectified spirit of wine and the nitrous acid of commerce, never failed, with me, to produce a fulminating mercury. It is indeed true, that the powder prepared without attention, is produced in different quantities, varies in colour, and probably in strength. From analogy, I am disposed to think the whitest is the strongest; for it is well known, that black precipitates of mercury approach the nearest to the metallic state. The variation in quantity is remarkable; the smallest quantity I ever obtained from 100 grains of quicksilver being 120 grains, and the largest 132 grains. Much depends on very minute circumstances. The greatest product seems to be obtained, when a vessel is used which condenses and causes most ether to return into the mother liquor; besides which, care is to be had in applying the requisite heat, that a speedy, and not a violent action be effected. 100 grains of an oxide are not so productive as 100 grains of quicksilver.

As to the colour, it seems to incline to black, when the action of the acid on the alcohol is most violent, and *vice versâ*.

SECTION X.

I need not observe, that the gases which were generated during the combustion of the powder in the glass globe, were necessarily mixed with atmospheric air; the facility wi h which the electric fluid passes through a vacuum, made such a mixture unavoidable.

The cubical inch of gas received over water was not readily

absorbed by it: and, as it soon extinguished a taper, without becoming red, or being itself inflamed, barytes water was let up to the three cubical inches received over mercury, when a carbonate of barytes was immediately precipitated.

The residue of several explosions, after the carbonic acid had been separated, was found, by the test of nitrous gas, to contain nitrogen or azotic gas; which does not proceed from any decomposition of atmospheric air, because the powder may be made to explode under the exhausted receiver of an air-pump. It is therefore manifest, that the gases generated during the combustion of the fulminating mercury, consist of carbonic acid and nitrogen gases.

SECTION XI.

The principal re-agents which decompose the mercurial powder, are the nitric, the sulphuric, and the muriatic acids. The nitric changes the whole into nitrous gas, carbonic acid gas, acetous acid, and nitrate of mercury. I resolved it into these different principles, by distilling it pneumatically with nitric acid: this acid, upon the application of heat, soon dissolved the powder, and extricated a quantity of gas, which was found, by well known tests, to be nitrous gas mixed with carbonic acid gas. The distillation was carried on until gas no longer came over. The liquor of the retort was then mixed with the liquor collected in the receiver, and the whole saturated with potash; which precipitated the mercury in a yellowish-brown powder, nearly as it would have done from a solution of nitrate of mercury. This precipitate was separated by a filter,

and the filtrated liquor evaporated to a dry salt, which was washed with alcohol. A portion of the salt being refused by this menstruum, it was separated by filtration, and recognized, by all its properties, to be nitrate of potash. The alcoholic liquor was likewise evaporated to a dry salt, which, upon the affusion of a little concentrate sulphuric acid, emitted acetous acid, contaminated with a feeble smell of nitrous acid, owing to the solubility of a small portion of the nitre in the alcohol.

SECTION XII.

The sulphuric acid acts upon the powder in a remarkable manner, as already has been noticed. A very concentrate acid produces an explosion nearly at the instant of contact, on account, I presume, of the sudden and copious disengagement of caloric from a portion of the powder which is decomposed by the acid. An acid somewhat less concentrate likewise extricates a considerable quantity of caloric, with a good deal of gas; but, as it effects a complete decomposition, it causes no explosion. An acid diluted with an equal quantity of water, by the aid of a little heat, separates the gas so much less rapidly, that it may with safety be collected in a pneumatic apparatus. But, whatever be the density of the acid, (provided no explosion be produced,) there remains in the sulphuric liquor, after the separation of the gas, a white uninflammable and uncrystallized powder, mixed with some minute globules of quicksilver.

To estimate the quantity, and observe the nature, of this uninflammable substance, I treated 100 grains of the fulminating mercury with sulphuric acid a little diluted. The gas being separated, I decanted off the liquor as it became clear, and freed the insoluble powder from acid, by edulcoration with distilled

water; after which, I dried it, and found it weighed only 84 grains; consequently had lost 16 grains of its original weight. Suspecting, from the operation of the nitric acid in the former experiment, that these 84 grains (with the exception of the quicksilver globules) were oxalate of mercury, I digested them in nitrate of lime, and found my suspicion just. The mercury of the oxalate united to the nitric acid, and the oxalic acid to the lime. A new insoluble compound was formed; it weighed, when washed and dry, 48,5 grains. Carbonate of potash separated the lime, and formed oxalate of potash, capable of precipitating lime-water, and muriate of lime; although it had been depurated from excess of alkali, and from carbonic acid, by a previous addition of acetous acid. That the mercury of the oxalate in the 84 grains, had united to the nitric acid of the nitrate of lime, was proved by dropping muriatic acid into the liquor from which the substance demonstrated to be oxalate of lime had been separated; for a copious precipitation of calomel instantly ensued.

The sulphuric liquor, decanted from the oxalate of mercury, was now added to that with which it was edulcorated, and the whole saturated with carbonate of potash. As effervescence ceased, a cloudiness and precipitation followed; and the precipitate, being collected, washed, and dried, weighed 3,4 grains: it appeared to be a carbonate of mercury. Upon evaporating a portion of the saturated sulphuric liquor, I found nothing but sulphate of potash; nor had it any metallic taste. There then remains, without allowing for the weight of the carbonic acid united to the 3,4 grains, a deficit from the 100 grains of mercurial powder, of 12,6 grains, which I ascribe to the gas separated by the action of the sulphuric acid. To ascertain the quantity, and examine the nature, of the gas so separated, I introduced into a

very small tubulated retort, 50 grains of the mercurial powder, and poured upon it 3 drams, by measure, of sulphuric acid, diluted with an equal quantity of water, and extricated the gas with the assistance of a gentle heat. I first received it over quicksilver, the surface of which, during the operation, partially covered itself with a little black powder.*

The gas, by different trials, amounted from 28 to 31 cubical inches; it at first appeared to be nothing but carbonic acid, as it precipitated barytes water, and extinguished a taper, without being itself inflamed, or becoming red. But, upon letting up to it liquid caustic ammoniac, there was a residue of from 5 to 7 inches of a peculiar inflammable gas, which burnt with a greenish blue flame. When I made use of the water-tub, I obtained, from the same materials, from 25 to 27 inches only of gas, although the average quantity of the peculiar inflammable gas was likewise from 5 to 7 inches; therefore, the difference of the aggregate product, over the two fluids, must have arisen from the absorption, by the water, of a part of the carbonic acid in its nascent state. The variation of the quantity of the inflammable gas, when powder from the same parcel is used, seems to depend upon the acid being a little more or less dilute.

With respect to the nature of the peculiar inflammable gas, it is plain to me, from the reasons I shall immediately adduce, that it is no other than the gas (in a pure state) into which the nitrous etherized gas can be resolved, by treatment with dilute sulphuric acid.

The Dutch chemists have shewn,† that the nitrous etherized gas can be resolved into nitrous gas, by exposure to concentrate

* I cannot account for this appearance.

† *Journal de Physique*, p. 250, October, 1794.

sulphuric acid, and that, by using a dilute instead of a concentrate acid, a gas is obtained which enlarges the flame of a burning taper, so much like the gaseous oxide of azote, that they mistook it for that substance, until they discovered that it was permanent over water, refused to detonate with hydrogen, and that the fallacious appearance was owing to a mixture of nitrous gas with an inflammable gas.

The inflammable gas separated from the powder answers to the description of the gas which at first deceived the Dutch chemists; 1st, in being permanent over water; 2dly, refusing to detonate with hydrogen; and, 3dly, having the appearance of the gaseous oxide of azote, when mixed with nitrous gas.

The gas separable by the same acid, from nitrous etherized gas, and from the mercurial powder, have therefore the same properties. Every chemist would thence conclude, that the nitrous etherized gas is a constituent part of the powder, had the inflammable and nitrous gas, instead of the inflammable and carbonic acid gas, been the mixed product extricated from it by dilute sulphuric acid.

It however appears to me, that nitrous gas was really produced by the action of the dilute sulphuric acid; and that, when produced, it united to an excess of oxygen present in the oxalate of mercury.

To explain how this change might happen, I must premise, that my experiments have shewn me, that oxalate of mercury can exist in two, if not in three states.

1st, By the discovery of Mr. AMEILON already quoted, the precipitate obtained by oxalic acid, from nitrate of mercury, fuses with a hissing noise. This precipitate is an oxalate of mercury, seemingly with excess of oxygen. Mercury dissolved in sul-

phuric acid and precipitated by oxalic acid, and also the pure red oxide of mercury digested with oxalic acid, give oxalates in the same state.

2dly. Acetate of mercury precipitated by oxalic acid, although a true oxalate is formed, has no kind of inflammability. I consider it as an oxalate with less oxygen than those abovementioned.

3dly. A solution of nitrate of mercury boiled with dulcified spirit of nitre, gives an oxalate more inflammable than any other: perhaps it contains most oxygen.

The oxalate of mercury remaining from the powder in the sulphuric liquor, is not only always in the same state as that precipitated from acetate of mercury, entirely devoid of inflammability, but contains globules of quicksilver; consequently, it must have parted with even more than its excess of oxygen; and, if nitrous gas was present, it would of course seize at least a portion of that oxygen. It is true, that globules of quicksilver may seem incompatible with nitrous acid; but the quantity of the one may not correspond with that of the other, or the dilution of the acid may destroy its action.

As to the presence of the carbonic acid, it must have arisen either from a complete* decomposition of a part of the oxalate; or, admitting the nitrous etherized gas to be a constituent principle of the powder, from a portion of the oxygen, not taken up by the nitrous gas, being united with the carbon of the etherized gas.

SECTION XIII.

The muriatic acid digested with the mercurial powder, dis-

* Inflammable oxalate of mercury, made to fuse in a retort connected with the quicksilver tub, gives out carbonic acid gas.

solves a portion of it, without extricating any notable quantity of gas. The dissolution evaporated to a dry salt, tastes like corrosive sublimate; and the portion which the acid does not take up, is left in the state of an uninflammable oxalate.

SECTION XIV.

These effects all tend to establish the existence of the nitrous etherized gas, as a constituent part of the powder; and likewise corroborate the explanation I have ventured to give, of the action of the sulphuric acid. Moreover, a measured ounce and a half of nitrous acid, holding 100 grains of mercury in solution, and 2 measured ounces of alcohol, yield 90 cubical inches only of gas: whereas, without the intervention of mercury, they yield 210 inches. Upon the whole, I trust it will be thought reasonable to conclude, that the mercurial powder is composed of the nitrous etherized gas, and of oxalate of mercury with excess of oxygen.

1st. Because the nitric acid converts the mercurial powder entirely into nitrous gas, carbonic acid gas, acetous acid, and nitrate of mercury.

2dly. Because the dilute sulphuric acid resolves it into an uninflammable oxalate of mercury, and separates from it a gas resembling that into which the same acid resolves the nitrous etherized gas.

3dly. Because an uninflammable oxalate is likewise left, after the muriatic acid has converted a part of it into sublimate.

4thly. Because it cannot be formed by boiling nitrate of mercury in dulcified spirit of nitre; although a very inflammable oxalate is by this means produced.

5thly. Because the difference of the product of gas, from the

same measures of alcohol and nitrous acid, with and without mercury in solution, is not trifling; and,

6thly. Because nitrogen gas was generated during its combustion in the glass globe.

Should my conclusions be thought warranted by the reasons I have adduced, the theory of the combustion of the mercurial powder will be obvious to every chemist. The hydrogen of the oxalic acid, and of the etherized gas, is first united to the oxygen of the oxalate, forming water;* the carbon is saturated with oxygen, forming carbonic acid gas; and a part, if not the whole of the nitrogen of the etherized gas, is separated in the state of nitrogen gas; both which last gases, it may be recollected, were after the explosion present in the glass globe. The mercury is revived, and, I presume, thrown into vapour; as may well be imagined, from the immense quantity of caloric extricated, by adding concentrate sulphuric acid to the mercurial powder.

I will not venture to state with accuracy, in what proportions its constituent principles are combined. The affinities I have brought into play are complicated, and the constitution of the substances I have to deal with not fully known. But, to make round numbers, I will resume the statement, that 100 grains of the mercurial powder lost 16 grains of its original weight, by treatment with dilute sulphuric acid: 84 grains of mercurial oxalate, mixed with a few minute globules of quicksilver, remained undissolved in the acid. The sulphuric liquor was saturated with carbonate of potash, and yielded 3,4 grains of carbonate of mercury. If 1,4 grain should be thought a proper

* Drops of water were observed on the internal surface of the globe, the day after several explosions had been produced in its centre.

allowance for the weight of carbonic acid in the 3,4 grains, I will make that deduction, and add the remaining 2 grains to the 84 grains of mercurial oxalate and quicksilver; I shall then have,

of oxalate and mercury - - -	86 grains
and a deficit, to be ascribed to the nitrous etherized gas and excess of oxygen - -	14
	100

It may perhaps be proper to proceed still further, and recur to the 48,5 grains, separated by nitrate of lime from the 84 grains of mercurial oxalate and globules of quicksilver, in the 11th section. These 48,5 grains were proved to be chiefly oxalate of lime; but they likewise contained a minute inseparable quantity of mercury, almost in the state of quicksilver, formerly part of the 84 grains from which they were separated. Had the 48,5 grains been pure calcareous oxalate, the quantity of pure oxalic acid in them would, according to BERGMANN,* be 23,28 grains. Hence, by omitting the 2 grains of mercury in the 3,4 grains of carbonate, 100 grains of the mercurial powder might have been said to contain, of pure oxalic acid 23,28 grains; of mercury 62,72 grains; and of nitrous etherized gas and excess of oxygen 14 grains. But, as the 48,5 grains were not pure oxalate, inasmuch as they contained the mercury they received from the 84 grains, from which they were generated by the nitrate of lime, some allowance must be made for the mercury successively intermixed with the 84 grains and the 48,5 grains.

In order to make corresponding numbers, and allow for unavoidable errors, I shall estimate the quantity of that mercury to have amounted to 2 grains, which I must of course deduct

* BERGMANN, *de Acido Saccbari*. Opuscula. Tom. I. § 6. p. 248. Leipzig, 1788.

from the 23,28 grains of oxalic acid. I shall then have the following statement:

That 100 grains of the fulminating mercury ought to contain,

of pure oxalic acid - - - -	21,28 grains,
of mercury formerly united to the oxalic acid	60,72
of mercury dissolved in the sulphuric liquor	2
and of mercury left in the sulphuric liquor after the separation of the gases - -	2
Total of mercury - -	64,72
Of nitrous etherized gas and excess of oxygen	14,
	100.

Since 100 grains of the powder seem to contain 64,72 grains of mercury, it will be immediately inquired, what becomes of 100 grains of quicksilver, when treated as directed, in the description of the process for preparing the fulminating mercury.

It has been stated (in section 9.) that 100 grains of quicksilver produce, under different circumstances, from 120 to 132 grains of mercurial powder; and, if 100 grains of this powder contain 64,72 grains, 120 grains, or 132 grains must, by parity of reasoning, contain 78,06 grains, or 85,47 grains: therefore, 13,34 grains, or 20,75 grains, more of the 100 grains are immediately accounted for; because 64,72 grains + 13,34 grains = 78,06, and 64,72 grains + 20,75 grains = 85,47 grains. The remaining deficiency of 21,94 grains, or 14,58 grains, which, with the 78,06 grains, or 85,47 grains, would complete the original 100, of quicksilver, remains partly in the liquor from which the powder is separated, and is partly volatilized in the white dense fumes, which in the beginning of this paper I compared to the *liquor fumans* of LIBAVIUS. The mercury cannot, in either

instance, be obtained in a form immediately indicative of its quantity; and a series of experiments to ascertain the quantities in which many different substances can combine with mercury, is not my present object. After observing, that the mercury left in the residuary liquor can be precipitated in a very subtle dark powder, by carbonate of potash, I shall content myself with examining the nature of the white fumes.

SECTION XV.

It is clear that these white fumes contain mercury: they may be wholly condensed in a range of Woulfe's apparatus, charged with a solution of muriate of ammoniac. When the operation is over, a white powder is seen floating with ether on the saline liquor, which, if the bottles are agitated, is entirely dissolved. After the mixture has been boiled, or for some time exposed to the atmosphere, it yields to caustic ammoniac a precipitate, in all respects similar to that which is separated by caustic ammoniac from corrosive sublimate.

I would infer from these facts, that the white dense fumes consist of mercury, or perhaps oxide of mercury, united to the nitrous etherized gas; and that, when the muriate of ammoniac containing them is exposed to the atmosphere, or is boiled, the gas separates from the mercury; and the excess of nitrous acid, which always comes over with nitrous ether, decomposes the ammoniacal muriate, and forms corrosive mercurial muriate or sublimate. This theory is corroborated, by comparing the quantity of gas estimated to be contained in the fulminating mercury, with the quantities of gas yielded from alcohol and nitrous acid, with and without mercury in solution; not to mention that more ether, as well as more gas, is produced without the inter-

vention of mercury; and that, according to the Dutch chemists, the product of ether, is always in the inverse ratio to the product of nitrous etherized gas. Should a further proof be thought necessary, of the existence of the nitrous etherized gas in the fulminating mercury, as well as in the white dense fumes, it may be added, that if a mixture of alcohol and nitrous acid holding mercury in solution, be so dilute, and exposed to a temperature so low, that neither ether nor nitrous etherized gas are produced, the fulminating mercury, or the white fumes, will never be generated: for, under such circumstances, the mercury is precipitated chiefly in the state of an inflammable oxalate. Further, when we consider the different substances formed by an union of nitrous acid and alcohol, we are so far acquainted with all, except the ether and the nitrous etherized gas, as to create a presumption, that no others are capable of volatilizing mercury, at the very low temperature in which the white fumes exist, since during some minutes they are permanent over water of 40° FAHRENHEIT.

SECTION XVI.

Hitherto, as much only has been said of the gas which is separated from the mercurial powder by dilute sulphuric acid, as was necessary to identify it with that into which the same acid can resolve the nitrous etherized gas: I have further to speak of its peculiarity.*

* It must be first noticed, that it is never pure when obtained from the nitrous etherized gas; nor am I aware how it is to be purified, unless the nitrous gas could be taken from it, without being converted into nitrous acid; for, by that acid, it would probably be itself converted into nitrous gas.

The characteristic properties of the inflammable gas, seem to me to be the following:

1st. It does not diminish in volume, either with oxygen or nitrous gas.

2dly. It will not explode with oxygen by the electric shock, in a close vessel.

3dly. It burns like hydrocarbonate, but with a bluish green flame. And,

4thly. It is permanent over water. (Section 12.)

It is of course either not formed, or is convertible into nitrous gas, by the concentrate nitric and muriatic acids; because, by those acids, no inflammable gas was extricated from the powder.

Should this inflammable gas prove not to be a hydrocarbonate, I shall be disposed to conclude, that it has nitrogen for its basis; indeed, I am at this moment inclined to that opinion, because I find that Dr. PRIESTLEY, during his experiments on his dephlogisticated nitrous air, once produced a gas which seems to have resembled this inflammable gas, both in the mode of burning, and in the colour of the flame.

After the termination of the common solution of iron in spirit of nitre, he used heat, and got, says he,* "such a kind "of air as I had brought nitrous air to be, by exposing it to "iron, or liver of sulphur; for, on the first trial, a candle "burned in it with a much enlarged flame. At another time, "the application of a candle to air produced in this manner, "was attended with a real though not a loud explosion; and, "immediately after this, a *greenish coloured flame descended* "*from the top to the bottom of the vessel* in which the air was

* PRIESTLEY on Air, Vol. II. p. 58. Birmingham. 1790.

"contained. In the next produce of air, from the same process, "*the flame descended blue* and very rapid, from the top to the "bottom of the vessel."

These greenish and blue coloured flames, descending from the top to the bottom of the vessel, are precisely descriptive of the inflammable gas separated from the powder. If it can be produced with certainty by the repetition of Dr. PRIESTLEY's experiments, or should it by any means be got pure from the nitrous etherized gas, my curiosity will excite me to make it the object of future research; otherwise, I must confess, I shall feel more disposed to prosecute other chemical subjects: for, having reason to think that the density of the acid made a variation in the product of this gas, and having never found that *any* acid, however dense, produced an immediate explosion, I once poured 6 drams of concentrate acid upon 50 grains of the powder. An explosion, nearly at the instant of contact, was effected: I was wounded severely, and most of my apparatus destroyed. A quantity moreover of the gas I had previously prepared, was lost by the inadvertency of a person who went into my laboratory, whilst I was confined by the consequences of this discouraging accident. But, should any one be desirous of giving the gas a further examination, I again repeat, that as far as I am enabled to judge, it may with safety be prepared, by pouring 3 drams of sulphuric acid diluted with the same quantity of water, upon 50 grains of the powder, and then applying the flame of a candle until gas begins to be extricated. The only attempt I have made to decompose it, was by exposing it to copper and ammoniac; which, during several weeks, did not effect the least alteration.

SECTION XVII.

I will now conclude, by observing, that the fulminating mercury seems to be characterised by the following properties.

It takes fire at the temperature of 368 FAHRENHEIT; explodes by friction,* by flint and steel, and by being thrown into concentrate sulphuric acid. It is equally inflammable under the exhausted receiver of an air-pump, as surrounded by atmospheric air; and it detonates loudly, both by the blow of a hammer, and by a strong electrical shock.

Notwithstanding the composition of fulminating silver, and of fulminating gold, differ essentially from that of fulminating mercury, all three have some similar qualities. In tremendous effects, silver undoubtedly stands first, and gold perhaps the last. The effects of the mercurial powder and of gunpowder, admit of little comparison. The one exerts, within certain limits, an almost inconceivable force: its agents seem to be gas and caloric, very suddenly set at liberty, and both mercury and water thrown into vapour. The other displays a more extended but inferior power: gas and caloric are, comparatively speaking, liberated by degrees; and water, according to Count RUMFORD, is thrown into vapour.†

Hence it seems, that the fulminating mercury, from the limitation of its sphere of action, can seldom if ever be applied to mining; and, from the immensity of its initial force, cannot be

* Consequently it should not be inclosed in a bottle with a glass stopper.

† See Philosophical Transactions, for the year 1797, p. 222.

The hard black substance mentioned by the Count, as remaining after the combustion of gunpowder, must, I believe, have been an alkaline sulphuret, mixed chiefly with sulphite and carbonate of potash. The conjecture that it is white when first formed, is certainly just, as my experiment with the glass globe evinced.

used in fire-arms, unless in cases where it becomes an object to destroy them; perhaps, where it is the practice to spike cannon, it may be of service, because, I apprehend, it may be used in such a manner as to burst cannon, without dispersing any splinters.

The inflammation of fulminating mercury by concussion, offers nothing more novel or remarkable, than the inflammation, by concussion, of many other substances. The theory of such inflammations has been long since exposed by the celebrated Mr. BERTHOLLET, and confirmed by Messieurs FOURCROY and VAUQUELIN: yet, I must confess, I am at a loss to understand, why a small quantity of mercurial powder made to detonate by the hammer, or the electric shock, should produce a report so much louder than when it is inflamed by a match, or by flint and steel. It might at first be imagined, that the loudness of the report could be accounted for, by supposing the instant of the inflammation, and that of the powder's confinement between the hammer and anvil, to be precisely the same; but, when the electrical shock is sent through or over a few grains of the powder, merely laid on ivory, and a loud report is the consequence, I can form no idea of what causes such a report.

The operation by which the powder is prepared, is perhaps one of the most beautiful and surprising in chemistry; and it is not a little interesting to consider the affinities which are brought into play. The superabundant nitrous acid of the mercurial solution, must first act on the alcohol, and generate ether, nitrous etherized gas, and oxalic acid. The mercury unites to the two last in their nascent state, and relinquishes fresh nitrous acid, to act upon any unaltered alcohol. The oxalic acid, although a predisposing affinity seems exerted in favour of its quantity, is evidently not formed fast enough to retain all the

mercury; otherwise, no white fumes, during a considerable period of the operation, but fulminating mercury alone, would be produced.

Should any doubt still be entertained of the existence of the affinities which have been called predisposing or conspiring, a proof that such affinities really exist, will I think be afforded, by comparing the quantity of oxalic acid which can be generated from given measures of nitrous acid and alcohol, with the intervention of mercury, and the intervention of other metals. For instance, when two measured ounces of alcohol are treated with a solution of 100 grains of nickel in a measured ounce and a half of nitrous acid, little or no precipitate is produced; yet, by the addition of oxalic acid to the residuary liquor, a quantity of oxalate of nickel, after some repose, is deposited. Copper affords another illustration: 100 grains of copper, dissolved in a measured ounce and a half of nitrous acid, and treated with alcohol, yielded me about 18 grains only of oxalate; although cupreous oxalate was plentifully generated, by dropping oxalic acid into the residuary liquor. About 21 grains of pure oxalic acid seem to be produced, from the same materials, when 100 grains of mercury are interposed. (See section 14.) Besides, according to the Dutch paper, more than once referred to, acetous acid is the principal residue after the preparation of nitrous ether. How can we explain the formation of a greater quantity of oxalic acid, from the same materials, with the intervention of 100 grains of mercury, than with the intervention of 100 grains of copper, otherwise than by the notion of conspiring affinities, so analogous to what we see in other phænomena of nature?

I have attempted, without success, to communicate fulminating properties, by means of alcohol, to gold, platina, antimony, tin, copper, iron, lead, zinc, nickel, bismuth, cobalt,

arsenic, and manganese; but I have not yet sufficiently varied my experiments, to enable me to speak with absolute certainty. Silver, when 20 grains of it were treated with nearly the same proportions of nitrous acid and alcohol as 100 grains of mercury, yielded, at the end of the operation, about 3 grains of a gray precipitate, which fulminated with extreme violence. Mr. CRUICKSHANK had the goodness to repeat the experiment: he dissolved 40 grains of silver in 2 ounces of the strongest nitrous acid diluted with an equal quantity of water, and obtained (by means of 2 ounces of alcohol) 60 grains of a very white powder, which fulminated like the gray precipitate above described. It probably combines with the same principles as the mercury, and of course differs from Mr. BERTHOLLET's fulminating silver, alluded to in page 230. I observe, that a white precipitate is always produced in the first instance, and that it may be preserved, by adding water, as soon as it is formed; otherwise, when the mother liquor is abundant, it often becomes gray, and is re-dissolved.

P. S. Since the preceding pages were written, I have been permitted, by the Right Honourable Lord HOWE, Lieutenant General of the Ordnance, to make the following trials of the mercurial powder, at Woolwich, in conjunction with Colonel BLOMEFIELD, and Mr. CRUICKSHANK.*

Experiment 1. From the manner in which the screw of the gun-breech, mentioned in Section v. had acted on the barrel, it was imagined, that by bursting an iron case, exactly fitted to the bore of a cannon, its sudden enlargement might make many flaws, and split the piece, without dispersing any splinters. In

* It is with pleasure I take this opportunity of acknowledging the civil attention I received from the different officers.

conformity to this opinion, a cast iron case was constructed, with a cylindrical chamber, of equal length and diameter, calculated to hold 34th ounces troy of the mercurial powder. The case, being firmly screwed together, was charged through its vent-hole, and introduced into a twelve-pounder carronade, the bore of which it exactly fitted. The powder was then enflamed, with proper precautions. The gun remained entire, but the case divided: the portion forming the upper surface of the chamber, was expelled in one mass; that adjoining the breech, which constituted the rest of the chamber, was cracked in every direction, and in part crumbled; yet it was so wedged into some indentations which the explosion had made in the sides of the piece, that the fragments were not removed without great labour.

Exp. 2. Another cast iron case was prepared, of the same size as the former, with a chamber also cylindrical, but wrought in a transverse direction, and of a greater length than diameter; the thickness of metal at each extremity not being more than a quarter of an inch. This case was filled with nearly 5 ounces troy of the mercurial powder, and placed in the same carronade. Three twelve-pound shot were next introduced, and brought into close contact with the upper surface of the case, as well as with each other. The gun a second time withstood the explosion: the case was divided across the middle of the chamber, into two equal parts; that adjoining the breech was, as in the former experiment, much flawed, and left immoveable; that nearest to the muzzle was also much flawed, but driven out with the shot. All the three shot were broken; the two lower being divided into several pieces, and the upper one cracked through the centre.

The report was so feeble, in both experiments, that an inattentive person, I am confident, would not have heard it at the distance of two hundred yards.

Exp. 3. It was found so difficult to extract the fragments of the case remaining in the carronade, after the last experiment, that a channel was drilled through them, to the vent-hole of the piece. It was then charged with 6 ounces troy of the mercurial powder, made up as a cartridge, which did not occupy above one half of the diameter of the bore. A wad was placed over the powder, dry sand superadded, to fill all vacuities, and the gun filled to the muzzle with two twelve-pound shot. A block of wood was set at a small distance, to receive the impression of the shot, and the powder was inflamed as usual. The carronade still resisted. One of the shot was split into two pieces; and the block of wood was driven to a considerable distance, but not penetrated by the shot above the depth of one inch. The report was somewhat louder than the former ones. In all three instances, a considerable recoil evidently took place. I presume, therefore, that in the first experiment related in the fifth Section, there must have been a recoil, though too trifling to be observed; and, in the instances where the gun and the proof were burst, it was not so much to be expected.

Exp. 4. Finding that the carronade, from the great comparative size of its bore to that of its length, required a larger quantity of mercurial powder to burst it than we were provided with, we charged a half-pounder swivel with an ounce and an half avoirdupois of the mercurial powder, (the service charge of gunpowder being 3 ounces,) and a half-pound shot between two wads. The piece was destroyed from the trunnions to the breech, and its fragments thrown thirty or forty yards. The ball penetrated five inches into a block of wood, standing at about a yard from the muzzle of the gun; the part of the swivel not broken, was scarce, if at all, moved from its original position.

Exp. 5. One ounce avoirdupois of the mercurial powder,

enclosed in paper, was placed in the centre of a shell 4,4 inches in diameter, and the vacant space filled with dry sand.

The shell burst by the explosion of the powder, and the fragments were thrown to a considerable distance. The charge of gunpowder employed to burst shells of this diameter, is 5 ounces avoirdupois.

Exp. 6. A sea grenade, 3,5 inches diameter, charged like the shell in the last experiment, was burst into numerous fragments, by $\frac{1}{4}$ of an ounce avoirdupois of the mercurial powder. The fragments were projected with but little force, and only to the distance of eight or ten yards. The charge of gunpowder required for grenades of this size, is 3 ounces.

Exp. 7. A sea grenade, of the same diameter as the last mentioned, and charged in the like manner, with $\frac{1}{8}$ of an ounce avoirdupois, or $57\frac{1}{2}$ grains, of the mercurial powder, was split into two equal pieces, which were not thrown ten inches asunder.

The report in the four last experiments was very sharp, but not loud in proportion.

It seems, from the manner in which the swivel was burst, in the fourth experiment, that a smaller charge would have been sufficient for the purpose. We may therefore infer, both from this instance and from the second experiment made with the gun, in Section v, that any piece of ordnance might be destroyed, by employing a quantity of the mercurial powder equal in weight to one half of the service charge of gunpowder; and, from the seventh and last experiment, we may also conclude, that it would be possible so to proportion the charge of mercurial powder to the size of different cannons, as to burst them without dispersing any splinters. But the great danger attending the use of fulminating mercury, on account of the facility with which it explodes, will probably prevent its being employed for that purpose.

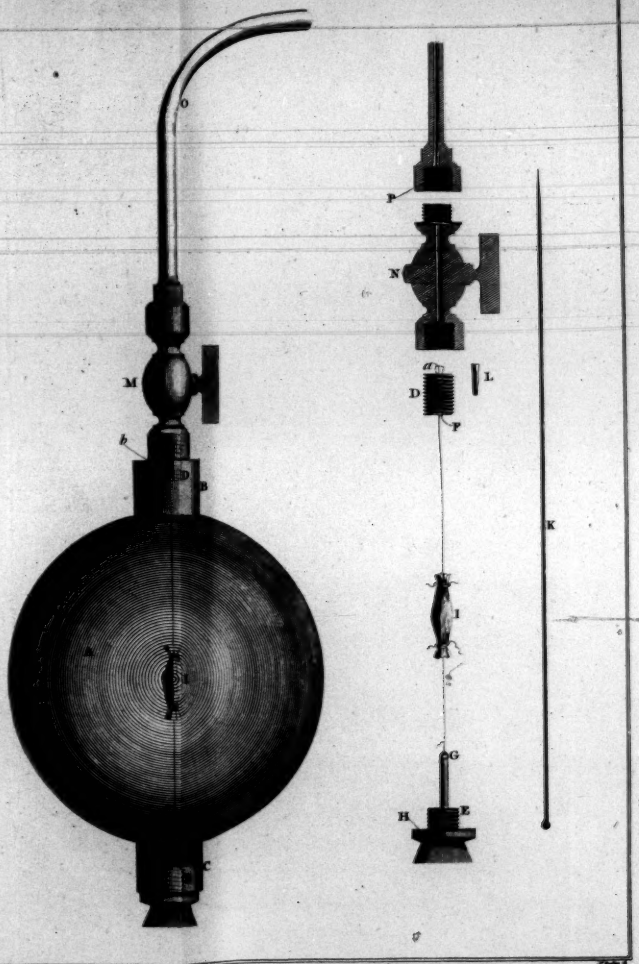
In addition to the other singular properties of the fulminating mercury, it may be observed, that two ounces inflamed in the open air, seem to produce a report much louder than when the same quantity is exploded in a gun capable of resisting its action. Mr. CRUICKSHANK, who made some of the powder, by my process, remarked that it would not inflame gunpowder. In consequence of which, we spread a mixture of coarse and fine grained gunpowder upon a parcel of the mercurial powder; and, after the inflammation of the latter, we collected most, if not all, of the grains of gunpowder. Can this extraordinary fact be explained by the rapidity of the combustion of fulminating mercury? or is it to be supposed, (as gunpowder will not explode at the temperature at which mercury is thrown into vapour,) that sufficient caloric is not extricated during this combustion?

From the late opportunity I have had of conversing with Mr. CRUICKSHANK, I find that he has made many accurate experiments on gunpowder; and he has permitted me to state, "that the matter which remains after the explosion of gun-"powder, consists of potash united with a small proportion of "carbonic acid, sulphate of potash, a very small quantity of "sulphuret of potash, and unconsumed charcoal. That 100 "grains of good gunpowder yield about 53 grains of this resi-"duum, of which three are charcoal. That it is extremely "deliquescent, and, when exposed to the air, soon absorbs "moisture sufficient to dissolve a part of the alkali; in conse-"quence of which, the charcoal becomes exposed, and the "whole assumes a black or very dark colour." Mr. CRUICK-SHANK likewise informs me, that after the combustion of good gunpowder under mercury, no water is ever perceptible.

REFERENCES TO THE FIGURES OF THE GLASS GLOBE, *&c.*
MENTIONED IN SECTION VII.

(See Plate VIII.)

A, a ball or globe of glass, nearly half an inch thick, and seven inches in diameter. It has two necks, on which are cemented the brass caps B, C, each being perforated with a female screw, to receive the male ones D, E: through the former a small hole is drilled; the latter is furnished with a perforated stud or shank G. By means of a leather collar H, the neck C can be air-tightly closed. When a portion of the powder is to be exploded, it must be placed on a piece of paper, and a small wire laid across the paper, through the midst of the powder: the paper being then closed, is to be tied at each end to the wire, with a silken thread, as shewn at I. One end of this wire is to be fastened to the end of the shank G, and the screw D inserted to half its length into the brass cap B; the other end of the wire, *a*, by means of the needle K, is to be drawn through the hole F. The screw E being now fixed in its place, and the wire drawn tight, it is to be secured, by pushing the irregular wooden plug L into the aperture of the screw D, taking care to leave a passage for air. The stop-cock M, the section of which is shewn at N, is now to be screwed on to the part D, which is made air-tight by the leather collar *b*. The glass tube O is bent, that it may more conveniently be introduced under the receiver of a pneumatic apparatus. P, shews the manner of connecting the glass tube with the stop-cock.


O
M
b
C
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K
I
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METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1799.

1799	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Jan. 1	30	8	0	30	41	30,34	79		E	1	Cloudy.
	32	2	0	32	43	30,34	77		NE	1	Cloudy.
2	30	8	0	30	43	30,43	75		E	1	Cloudy.
	32	2	0	31	43	30,43	74		NE	1	Cloudy.
3	27	8	0	27	42	30,40	80		NE	1	Cloudy.
	32	2	0	32	44	30,38	76		NE	1	Cloudy.
4	30	8	0	30	43	30,35	78		NE	1	Cloudy.
	33	2	0	33	44	30,35	75		NE	1	Cloudy.
5	30	8	0	30	43	30,32	78		E	1	Cloudy.
	31	2	0	31	44	30,28	76		E	1	Cloudy.
6	27	8	0	28	42	30,22	78		WSW	1	Cloudy.
	36	2	0	36	46	30,21	77		NNW	1	Fine.
7	26	8	0	27	43	30,24	78		W	1	Cloudy.
	35	2	0	35	46	30,14	78		SW	1	Cloudy.
8	33	8	0	36	44	29,96	86	0,122	ESE	1	Foggy.
	38	2	0	38	47	30,03	87		SE	1	Cloudy.
9	32	8	0	33	46	30,31	84	0,033	NE	1	Fine.
	34	2	0	34	47	30,32	85		NE	1	Foggy.
10	31	8	0	31	46	30,26	84		E	1	Cloudy.
	38	2	0	38	49	30,22	83		S	1	Cloudy.
11	35	8	0	38	48	30,14	86		SW	1	Foggy.
	41	2	0	41	50	30,16	84		W	1	Cloudy.
12	30	8	0	30	47	30,23	83				Foggy.
	33	2	0	32	50	30,25	82		SW	1	Cloudy.
13	32	8	0	35	48	30,21	84		SSW	1	Cloudy.
	40	2	0	40	50	30,21	80		S	1	Cloudy.
14	35	8	0	35	48	30,25	81		SSE	1	Cloudy.
	41	2	0	41	50	30,29	78		SE	1	Cloudy.
15	38	8	0	38	49	30,35	80		SSE	1	Cloudy.
	42	2	0	42	52	30,32	66		SSE	1	Cloudy.
16	28	8	0	30	49	30,22	78		E	1	Fair.
	39	2	0	39	53	30,20	75		SE	1	Fine.

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for January, 1799.

1799	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Jan. 17	31	8	0	31	50	30,19	71		E	1	Cloudy.
	34	2	0	34	51	30,15	68		E	1	Fair.
18	30	8	0	33	50	29,97	70		ESE	1	Cloudy.
	43	2	0	43	53	29,93	78		S	1	Cloudy.
19	41	8	0	42	51	30,01	88		S	2	Cloudy.
	43	2	0	43	53	30,00	80		SSE	2	Cloudy.
20	35	8	0	42	50	29,91	73		SSE	2	Fair.
	44	2	0	44	52	29,82	70		SSE	2	Cloudy.
21	42	8	0	43	50	29,62	86		SSE	2	Cloudy.
	45	2	0	45	53	29,62	85		SSE	2	Cloudy.
22	34	8	0	39	50	29,82	80	0,169	S	2	Cloudy.
	45	2	0	45	53	29,77	77		S	2	Fair.
23	40	8	0	41	51	29,25	78	0,077	S	1	Cloudy.
	44	2	0	44	54	29,25	74		S	2	Cloudy.
24	35	8	0	35	51	29,52	76	0,082	SSW	1	Fair.
	45	2	0	44	55	29,55	67		SSW	1	Fair.
25	35	8	0	37	52	29,70	75		SSW	1	Cloudy.
	42	2	0	42	54	29,65	75		NE	1	Cloudy.
26	34	8	0	36	52	29,83	76		SW	1	Cloudy.
	45	2	0	45	54	29,90	65		WNW	1	Cloudy.
27	37	8	0	40	52	29,58	78	0,161	SSE	1	Cloudy.
	50	2	0	50	54	29,41	85		SSW	1	Rain.
28	32	8	0	32	53	29,48	80	0,305	ESE	1	Snow.
	32	2	0	29	54	29,64	61		NW	1	Fair.
29	25	8	0	26	52	29,75	75		W	1	Cloudy.
	34	2	0	34	54	29,68	65		S	1	Fine.
30	29	8	0	29	50	29,37	77		NE	1	Snow.
	31	2	0	31	52	29,46	73		NE	1	Fine.
31	20	8	0	23	48	29,55	72		E	1	Cloudy.
	28	2	0	28	48	29,26	80		E	2	Snow.

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for February, 1799.

1799	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Feb. 1	26	7	0	30	48	29,24	92		E	1	Snow.
	33	2	0	32	50	29,22	91		E	1	Snow.
2	29	7	0	29	48	29,09	90		NE	1	Cloudy.
	31	2	0	31	50	29,11	82		NE	1	Snow.
3	22	7	0	22	46	29,48	73		W	1	Fine.
	27	2	0	27	49	29,53	68		WNW	2	Fair.
4	18	7	0	22	46	29,57	74		NE	1	Cloudy.
	30	2	0	30	48	29,56	75		NE	1	Cloudy.
5	19	7	0	25	45	29,59	77		NE	1	Cloudy.
	30	2	0	29	47	29,57	74		E	1	Cloudy.
6	25	7	0	25	44	29,65	70		E	1	Cloudy.
	28	2	0	28	46	29,70	69		ENE	2	Cloudy.
7	25	7	0	25	44	29,87	71		NE	1	Cloudy.
	28	2	0	27	44	29,92	69		NE	1	Cloudy.
8	23	7	0	23	43	30,09	68		NE	1	Cloudy.
	27	2	0	26	45	30,14	68		E	1	Cloudy.
9	23	7	0	30	42	29,84	84		ESE	2	Snow.
	40	2	0	39	47	29,88	84		SSW	1	Fair.
10	32	7	0	33	45	30,26	86		SSW	1	Fair.
	42	2	0	40	48	30,26	82		SSW	1	Cloudy.
11	37	7	0	40	46	29,18	84	0,953	S	2	Cloudy.
	41	2	0	39	49	28,88	78		SSE	2	Rain.
12	30	7	0	30	45	29,70	68	0,115	NE	2	Fine.
	35	2	0	34	49	29,93	67		NNE	2	Fine.
13	30	7	0	32	46	29,82	82	0,020	SSW	2	Snow.
	41	2	0	40	49	29,80	78		W	1	Cloudy.
14	32	7	0	32	47	29,90	77		W	1	Fair.
	44	2	0	44	50	29,88	66		SSW	1	Fair.
15	40	7	0	43	50	29,48	87	0,088	WSW	1	Cloudy.
	50	2	0	50	54	29,45	67		SSW	2	Cloudy.
16	38	7	0	40	51	29,07	78	0,380	SW	1	Cloudy.
	45	2	0	44	53	29,12	70		WSW	1	Rain.

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1799	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Feb. 17	37	7	0	37	51	29,40	75		WSW	1	Cloudy.
	44	2	0	43	53	29,51	63		WNW	2	Fair.
18	37	7	0	37	52	29,70	75	0,065	E	1	Rain.
	48	2	0	42	54	29,61	80		E	1	Rain.
19	41	7	0	49	53	29,54	89	0,205	SSW	2	Cloudy.
	53	2	0	50	56	29,51	82		S	2	Rain.
20	42	7	0	44	53	29,44	83	0,115	SE	1	Cloudy.
	49	2	0	47	56	29,44	76		SW	1	Cloudy.
21	44	7	0	47	54	29,40	83	0,102	SSW	2	Rain.
	53	2	0	52	58	29,48	67		SSW	2	Fair.
22	39	7	0	47	55	29,75	72	0,188	SSW	2	Cloudy.
	56	2	0	56	58	29,91	69		SSW	2	Cloudy.
23	44	7	0	45	56	30,00	74		E	1	Fair.
	56	2	0	56	60	29,98	67		SSE	1	Fair.
24	48	7	0	48	57	30,00	77		S	1	Cloudy.
	55	2	0	55	59	30,03	63		S	1	Fair.
25	39	7	0	39	56	30,18	73		SSW	1	Fair.
	47	2	0	46	59	30,26	57		NW	1	Fine.
26	37	7	0	40	56	30,15	69		SW	1	Cloudy.
	50	2	0	50	59	30,13	70		WSW	1	Cloudy.
27	47	7	0	49	57	29,94	80	0,104	W8W	1	Cloudy.
	56	2	0	56	59	29,95	74		W	1	Cloudy.
28	43	7	0	44	57	30,15	76		SW	1	Cloudy.
	54	2	0	54	59	30,13	69		SSW	1	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Mar. 1	47	7	0	47	58	30,03	79		S	1	Cloudy.
	56	2	0	56	61	29,90	66		SSE	1	Fair.
2	42	7	0	42	59	29,93	75		WNW	1	Fair.
	50	2	0	50	62	30,05	53		NE	1	Fine.
3	36	7	0	36	58	30,21	68		E	1	Fair.
	49	2	0	49	60	30,20	55		E	1	Fine.
4	32	7	0	33	56	30,19	74		E	1	Fair.
	48	2	0	48	59	30,14	63		E	1	Fine.
5	35	7	0	36	56	30,17	71		E	2	Cloudy.
	41	2	0	41	58	30,21	62		E	2	Fair.
6	35	7	0	35	55	30,23	65		NE	1	Cloudy.
	37	2	0	37	56	30,21	64		NE	2	Cloudy.
7	33	7	0	33	53	30,20	67		NNW	1	Snow.
	37	2	0	37	54	30,19	68		N	1	Cloudy.
8	32	7	0	34	53	30,07	73		NE	1	Cloudy.
	38	2	0	37	54	29,99	70		NE	1	Cloudy.
9	29	7	0	31	51	29,92	74		NE	1	Snow.
	35	2	0	35	54	29,94	72		NE	1	Snow.
10	32	7	0	32	51	30,11	71		E	1	Cloudy.
	42	2	0	41	54	30,16	64		E	1	Fair.
11	32	7	0	32	50	30,05	76		NE	1	Cloudy.
	40	2	0	39	52	30,01	73		NE	1	Cloudy.
12	36	7	0	36	51	30,04	77		NE	1	Cloudy.
	48	2	0	47	54	30,07	70		NE	1	Fair.
13	33	7	0	33	51	29,84	70		NW	1	Cloudy.
	44	2	0	44	54	29,55	66		WNW	1	Cloudy.
14	32	7	0	33	50	29,51	74	0,250	NE	2	Snow.
	36	2	0	36	52	29,54	73		NE	2	Cloudy.
15	34	7	0	34	50	29,74	72	0,021	NE	2	Cloudy.
	40	2	0	40	52	29,77	60		NE	2	Cloudy.
16	33	7	0	33	50	29,80	68		E	1	Cloudy.
	42	2	0	42	52	29,87	57		NE	1	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	o			o	o	Inches.		Inches.			
Mar. 17	35	7	o	36	50	29,78	62		S	1	Cloudy.
	42	2	o	42	51	29,72	58		S	1	Cloudy.
18	34	7	o	34	49	29,67	63		SSE	1	Cloudy.
	45	2	o	45	52	29,69	61		S	1	Cloudy.
19	31	7	o	31	50	29,81	75		W	1	Fair.
	45	2	o	45	54	29,66	56		S	1	Cloudy.
20	38	7	o	38	50	29,31	74	0,023	E	1	Cloudy.
	42	2	o	42	54	29,38	76		ESE	1	Cloudy.
21	33	7	o	33	49	29,68	75		NE	1	Cloudy.
	46	2	o	46	52	29,70	70		ENE	1	Cloudy.
22	36	7	o	36	51	29,72	76		NE	1	Rain.
	41	2	o	41	53	29,72	76		ENE	1	Cloudy.
23	38	7	o	38	52	29,70	*	0,062	NE	1	Cloudy.
	46	2	o	46	54	29,68			S	1	Fair.
24	40	7	o	43	52	29,62		0,028	E	1	Cloudy.
	54	2	o	54	55	29,55			SE	2	Fair.
25	46	7	o	46	54	29,45			ESE	2	Cloudy.
	50	2	o	50	56	29,59			W	1	Fair.
26	36	7	o	38	54	29,74		0,018	SSE	2	Fair.
	45	2	o	43	55	29,60			S	2	Rain.
27	37	7	o	40	54	29,51		0,031	SW	2	Fair.
	51	2	o	50	56	29,49			WSW	1	Fair.
28	34	7	o	35	54	29,55			SW	1	Cloudy.
	49	2	o	49	57	29,66			WSW	1	Fair.
29	34	7	o	34	54	29,95			SW	1	Fair.
	48	2	o	48	56	29,97			E	1	Fair.
30	31	7	o	31	51	30,00			E	2	Cloudy.
	34	2	o	34	54	29,94			E	2	Fair.
31	28	7	o	28	50	29,77			ENE	2	Cloudy.
	34	2	o	34	52	29,73			ENE	2	Fair.

* The whalebone of the hygrometer slipped out of its place.

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1799	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	o		o	o						
April 1	28	7 0	30	48	29,77			NE	2	Fair.
	37	2 0	37	54	29,83			NE	2	Fine.
2	31	7 0	32	47	29,92			NE	2	Cloudy.
	38	2 0	38	51	29,97			NE	2	Fair.
3	30	7 0	31	48	29,85			E	2	Cloudy.
	34	2 0	34	49	29,82			E	2	Fair.
4	27	7 0	31	48	29,78			E	2	Fair.
	44	2 0	44	52	29,70			ESE	2	Fine.
5	39	7 0	42	50	29,20		0,312	E	2	Rain.
	53	2 0	53	54	29,24			SSE	2	Cloudy.
6	42	7 0	42	53	29,40		0,068	NE	1	Cloudy.
	45	2 0	45	54	29,48			NE	1	Cloudy.
7	35	7 0	36	52	29,76			NE	1	Cloudy.
	41	2 0	41	53	29,79			NE	1	Cloudy.
8	37	7 0	38	50	29,66			E	1	Cloudy.
	52	2 0	52	54	29,55			SSE	1	Cloudy.
9	40	7 0	43	52	29,55		0,167	S	2	Fine.
	54	2 0	54	57	29,54			S	2	Cloudy.
10	45	7 0	46	53	28,86		0,100	S	2	Rain. [Much wind last night.
	49	2 0	49	55	28,75			S	2	Rain.
11	38	7 0	42	53	29,27		0,062	SW	2	Fair.
	54	2 0	54	56	29,27			SSW	2	Cloudy.
12	40	7 0	42	54	29,36		0,039	SSW	1	Cloudy.
	50	2 0	50	56	29,55			NE	1	Cloudy.
13	41	7 0	41	55	30,01		0,036	NE	1	Cloudy.
	50	2 0	50	56	30,12			NE	1	Fair.
14	34	7 0	36	54	30,23			NE	1	Fine.
	47	2 0	47	54	30,11			NE	1	Cloudy.
15	41	7 0	41	53	29,55			E	2	Cloudy.
	59	2 0	56	57	29,42			E	2	Fine.
16	45	7 0	48	55	29,21		0,075	E	1	Cloudy.
	55	2 0	55	57	29,26			SW	1	Fair.

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1799	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	o		o	o						
Apr. 17	45	7 0	47	55	29,15		0,073	SSE	1	Cloudy.
	54	2 0	54	58	29,22			S	2	Cloudy.
18	39	7 0	41	55	29,02		0,076	SSW	2	Fair.
	51	2 0	51	58	29,00			SW	2	Fair.
19	39	7 0	43	55	28,95		0,185	ESE	1	Rain.
	50	2 0	50	57	29,08			W	1	Cloudy.
20	41	7 0	42	55	29,38		0,170	NNE	2	Cloudy.
	50	2 0	50	56	29,52			NW	2	Cloudy.
21	40	7 0	42	55	29,75		0,018	NE	2	Cloudy.
	51	2 0	50	58	29,82			NE	2	Cloudy.
22	39	7 0	42	55	29,79			S	1	Cloudy.
	52	2 0	50	57	29,66			S	2	Rain.
23	46	7 0	46	55	29,41		0,086	ENE	1	Rain.
	52	2 0	51	57	29,51			ENE	1	Cloudy.
24	41	7 0	41	53	29,87		0,028	NE	1	Cloudy.
	48	2 0	48	56	29,92			NE	1	Cloudy.
25	37	7 0	38	54	30,06			NE	1	Cloudy.
	49	2 0	49	56	30,03			NE	1	Cloudy.
26	42	7 0	42	55	29,78		0,176	NE	1	Cloudy.
	47	2 0	47	57	29,96			NE	1	Cloudy.
27	39	7 0	41	55	29,98			NE	1	Cloudy.
	53	2 0	53	57	29,97			NE	1	Cloudy.
28	48	7 0	49	55	29,90			NW	1	Cloudy.
	56	2 0	56	57	29,89			NW	1	Cloudy.
29	42	7 0	42	55	29,96			NE	1	Cloudy.
	49	2 0	47	56	29,96			NE	1	Cloudy.
30	41	7 0	42	55	30,04			NE	1	Cloudy.
	48	2 0	48	56	30,02			NE	1	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
May 1	37	7 0	40	54	29.94			S	1	Cloudy.
	57	2 0	57	56	29.83			WSW	1	Fair.
2	43	7 0	43	54	29.61		0,191	NE	1	Rain.
	47	2 0	47	56	29,61			E	1	Cloudy.
3	39	7 0	41	55	29,74			NE	1	Cloudy.
	52	2 0	52	57	29.74			NE	1	Cloudy.
4	39	7 0	41	55	29,85			ENE	1	Fair.
	52	2 0	52	56	29,83			NE	1	Cloudy.
5	44	7 0	42	56	29,73			NE	2	Cloudy.
	54	2 0	54	57	29,69			E	2	Fair.
6	48	7 0	50	56	29,63		0,052	E	1	Cloudy.
	63	2 0	63	59	29,63			E	1	Fair.
7	44	7 0	44	57	29,52		0,137	NE	1	Rain.
	61	2 0	61	60	29,48			E	1	Fair.
8	48	7 0	48	58	29,50		0,061	NE	1	Cloudy.
	59	2 0	58	60	29,51			NE	1	Cloudy.
9	51	7 0	52	59	29,58		0,053	SW	1	Cloudy.
	59	2 0	58	60	29,58			SSW	1	Cloudy.
10	47	7 0	48	59	29,58		0,020	SSW	1	Rain.
	57	2 0	57	59	29.55			WSW	1	Cloudy.
11	44	7 0	46	59	29,55			SW	1	Fair.
	60	2 0	54	60	29,51			SE	1	Rain.
12	45	7 0	48	58	29,61		0,023	NE	1	Fair.
	61	2 0	55	59	29,67			NE	1	Rain.
13	48	7 0	48	60	29,71		0,054	SSE	1	Cloudy.
	58	2 0	57	60	29,68			SSE	1	Fair.
14	46	7 0	48	59	29,71		0,080	NE	1	Cloudy.
	53	2 0	51	60	29,77			NE	1	Cloudy.
15	38	7 0	42	57	29.97			NE	2	Cloudy.
	51	2 0	51	58	30,02			NE	2	Cloudy.
16	36	7 0	40	57	30,34			NE	2	Fine.
	55	2 0	55	58	30,38			NE	2	Fair.

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1799	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
May 17	41	7	0	46	57	30,33			S	1	Fair.
	61	2	0	61	58	30,19			SW	1	Hazy.
18	47	7	0	49	57	29,81		0,053	SSW	1	Cloudy.
	66	2	0	66	60	29,71			SSW	1	Fair.
19	51	7	0	53	58	29,47		0,117	W	1	Cloudy.
	63	2	0	61	59	29,57			SW	1	Cloudy.
20	51	7	0	51	58	29,33		0,575	NE	1	Rain.
	57	2	0	57	60	29,64			N	2	Cloudy.
21	45	7	0	48	59	29,88		0,245	W	2	Hazy.
	55	2	0	55	60	29,76			SSW	2	Cloudy.
22	47	7	0	50	59	29,97			WNW	2	Fine.
	64	2	0	64	61	30,07			WNW	2	Fair.
23	54	7	0	57	60	29,94			W	2	Cloudy.
	65	2	0	65	61	29,95			W	2	Fair.
24	45	7	0	49	60	29,94		0,016	W	2	Cloudy.
	60	2	0	60	60	30,05			NW	2	Fair.
25	45	7	0	49	59	30,32			WNW	1	Fair.
	65	2	0	65	61	30,26			W	1	Fine.
26	47	7	0	51	60	30,17			W	2	Fair.
	66	2	0	66	61	30,10			W	2	Cloudy.
27	48	7	0	51	60	30,16			NE	1	Fine.
	64	2	0	64	61	30,20			N	1	Fair.
28	46	7	0	51	60	30,27			ENE	1	Hazy.
	59	2	0	59	61	30,27			E	1	Fine.
29	45	7	0	51	60	30,10			E	1	Fine.
	70	2	0	70	62	29,96			E	1	Fine.
30	53	7	0	55	61	29,81			SSW	1	Cloudy.
	60	2	0	58	61	29,80			NW	1	Rain.
31	48	7	0	52	60	30,03		0,072	NW	1	Cloudy.
	66	2	0	65	61	30,07			WNW	1	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
June 1	48	7	0	52	60	29,99			SW	2	Fair.
	60	2	0	58	61	29,88			S	2	Rain.
2	49	7	0	50	60	29,76		0,028	S	1	Cloudy.
	61	2	0	59	60	29,68			SW	2	Fair.
3	50	7	0	52	60	29,55		0,055	S	2	Cloudy.
	61	2	0	61	60	29,54			SW	2	Fair.
4	50	7	0	53	60	29,18		0,056	SW	2	Rain.
	58	2	0	58	59	29,30			SW	2	Fair.
5	46	7	0	49	58	29,68			SW	2	Fair.
	62	2	0	61	60	29,87			NW	1	Fair.
6	48	7	0	52	59	30,19			SW	1	Cloudy.
	67	2	0	67	60	30,22			SW	1	Fair.
7	55	7	0	57	60	30,38			SW	1	Cloudy.
	75	2	0	75	62	30,41			SSW	1	Cloudy.
8	58	7	0	61½	62	30,41			E	1	Fine.
	71	2	0	71	64	30,35			E	1	Fine.
9	55	7	0	61	64	30,20			ENE	1	Fine.
	74	2	0	74	65	30,12			NE	1	Fine.
10	57	7	0	61	65	30,05			NE	1	Fair.
	77	2	0	77	67	30,03			NE	1	Fine.
11	49	7	0	51	65	30,21			NE	2	Fine.
	59	2	0	59	65	30,21			NE	2	Fine.
12	45	7	0	51	63	30,26			NE	2	Cloudy.
	64	2	0	64	64	30,15			NE	1	Fine.
13	52	7	0	56	64	29,94			NW	1	Cloudy.
	64	2	0	61	64	29,82			NW	2	Cloudy.
14	53	7	0	56	63	29,86			NE	1	Cloudy.
	65	2	0	64	63	29,90			NE	1	Cloudy.
15	46	7	0	50	62	30,02			NE	1	Fair.
	60	2	0	60	62	30,05			NE	1	Cloudy.
16	43	7	0	50	61	30,15			NE	1	Cloudy.
	59	2	0	59	61	30,17			NE	1	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	o			o	o	Inches.		Inches.			
June 17	45	7	o	50	60	30,17			NE	1	Cloudy.
	61	2	o	61	62	30,15			NE	1	Fine.
18	47	7	o	51	60	30,21			NE	1	Cloudy.
	64	2	o	64	62	30,20			NE	1	Fine.
19	49	7	o	53	61	30,23			NE	1	Cloudy.
	67	2	o	67	62	30,23			NE	1	Fine.
20	47	7	o	51	62	30,25			NE	1	Cloudy.
	62	2	o	62	62	30,22			NE	1	Fair.
21	50	7	o	53	62	30,20			NE	1	Cloudy.
	65	2	o	65	64	30,20			NE	1	Fair.
22	52	7	o	54	62	30,20			NE	1	Fair.
	71	2	o	71	63	30,11			NE	1	Fair.
23	54	7	o	60	63	29,90			SE	1	Fine.
	72	2	o	72	63	29,81			SW	1	Cloudy.
24	47	7	o	50	62	30,00		0,033	NE	2	Fair.
	67	2	o	67	63	30,00			NE	1	Fair.
25	46	7	o	51	62	30,15			NE	1	Fair.
	66	2	o	63	62	30,10			E	1	Fair.
26	54	7	o	54	62	29,98		0,025	W	1	Rain.
	61	2	o	59	62	29,95			S	1	Cloudy.
27	56	7	o	56	62	30,01			W	1	Cloudy.
	66	2	o	66	62	30,05			NE	1	Cloudy.
28	58	7	o	59	63	30,18			NW	1	Cloudy.
	67	2	o	67	63	30,21			NW	1	Rain.
29	55	7	o	56	63	30,24		0,355	NE	1	Cloudy.
	67	2	o	67	64	30,21			NE	1	Cloudy.
30	49	7	o	53	63	30,14			SW	1	Fine.
	77	2	o	77	64	30,04			SSW	1	Fine.

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1799	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
	o										
July 1	59	7	o	60	64	29.90			SW	1	Cloudy.
	72	2	o	72	64	29,86			S	1	Cloudy.
2	59	7	o	59	64	29,83			S	1	Rain.
	72	2	o	72	65	29,86			W	1	Fair.
3	58	7	o	59	65	29,97			SSE	1	Cloudy.
	72	2	o	72	66	29,97			ESE	1	Fair.
4	60	7	o	57	65	30,05			SW	1	Fine.
	74	2	o	73	67	30,04			SW	1	Fair.
5	60	7	o	62	66	30,10			SW	1	Fair.
	74	2	o	74	67	30,12			SSW	1	Fair.
6	60	7	o	61	67	30,18			SW	1	Cloudy.
	77	2	o	77	67	30,12			SSW	2	Cloudy.
7	59	7	o	63	67	30,10			SSW	1	Cloudy.
	74	2	o	74	67	30,07			SW	1	Fair.
8	57	7	o	60	68	30,03			SW	1	Fine.
	77	2	o	77	68	29,98			SW	1	Cloudy.
9	63	7	o	63	68	29,94		0,530	S	1	Rain.
	65	2	o	65	67	29,92			NW	1	Cloudy.
10	55	7	o	58	67	30,01		0,135	E	1	Fair.
	70	2	o	70	68	29,98			E	1	Fine.
11	56	7	o	59	67	29,89			SE	1	Fine.
	70	2	o	70	67	29,77			SSE	2	Cloudy.
12	61	7	o	62	67	29,54		0,120	S	1	Cloudy.
	73	2	o	73	68	29,52			S	2	Fair.
13	53	7	o	56	66	29,77		0,110	W	2	Fine.
	69	2	o	69	68	29,73			SW	2	Cloudy.
14	50	7	o	54	65	29,94		0,093	WNW	2	Fine.
	70	2	o	70	66	29,98			WNW	2	Fair.
15	57	7	o	58	65	29,58			S	2	Cloudy.
	65	2	o	65	65	29,61			W	2	Cloudy.
16	52	7	o	55	64	29,88			NE	1	Cloudy.
	60	2	o	57	64	29,91			N	1	Rain.

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1799	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	o									
July 17	48	7 0	52	63	29,97		0,081	SW	1	Fair.
	69	2 0	69	64	29,87	38		S	2	Fair.
18	53	7 0	55	63	29,33	72	0,386	ESE	2	Rain.
	68	2 0	68	64	29,22	59		SE	1	Cloudy.
19	57	7 0	58	63	29,35	65	0,175	SW	1	Cloudy.
	63	2 0	63	64	29,47	63		NW	1	Cloudy.
20	50	7 0	54	63	29,74	65	0,016	W	1	Cloudy.
	68	2 0	68	64	29,81	42		W	1	Fair.
21	53	7 0	56	63	29,86	61		SW	1	Cloudy.
	66	2 0	66	64	29,83	45		SW	1	Fair.
22	53	7 0	55	63	29,91	64		SW	1	Fair.
	68	2 0	68	64	29,82	51		SE	1	Cloudy.
23	57	7 0	60	64	29,53	70	0,038	SW	1	Cloudy.
	71	2 0	71	65	29,56	46		SW	1	Cloudy.
24	54	7 0	57	63	29,48	73	0,695	S	1	Rain.
	65	2 0	63	64	29,48	59		S	2	Cloudy.
25	54	7 0	57	64	29,55	63		S	1	Fair.
	69	2 0	67	64	29,64	54		NW	1	Cloudy.
26	54	7 0	56	63	29,94	65	0,041	WSW	1	Cloudy.
	67	2 0	67	64	29,94	49		ENE	1	Fair.
27	54	7 0	57	64	29,82	64		E	2	Cloudy.
	65	2 0	65	64	29,71	57		E	2	Cloudy.
28	57	7 0	58	64	29,53	80	0,475	E	1	Rain.
	64	2 0	62	64	29,61	69		NE	1	Cloudy.
29	51	7 0	53	63	29,88	61	0,018	NW	1	Fair.
	66	2 0	65	63	29,90	48		N	1	Cloudy.
30	54	7 0	56	63	30,00	61		WNW	1	Cloudy.
	68	2 0	67	63	30,04	45		NW	1	Cloudy.
31	55	7 0	56	62	29,98	62		SSW	1	Cloudy.
	70	2 0	70	64	29,90	48		S	2	Fair.

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1799	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Aug. 1	57	7 0	58	63	29,78	63		SW	1	Cloudy.
	73	2 0	72	64	29,80	45		SSW	1	Fair.
2	53	7 0	57	64	29,97	65		S	1	Cloudy.
	70	2 0	68	64	29,91	49		S	2	Cloudy.
3	59	7 0	61	64	29,63	73	0,180	S	1	Cloudy.
	71	2 0	69	66	29,61	47		S	2	Fair.
4	54	7 0	57	65	29,73	63		SW	2	Fine.
	72	2 0	71	65	29,71	47		SSW	2	Fair.
5	57	7 0	57	64	29,36	70	0,120	SE	2	Rain.
	69	2 0	68	65	29,38	52		SW	2	Cloudy.
6	54	7 0	56	64	29,76	64		SSW	1	Cloudy.
	68	2 0	60	64	29,76	63		SE	1	Rain.
7	60	7 0	62	65	29,68	78	0,275	S	1	Cloudy.
	68	2 0	64	65	29,88	62		WSW	1	Cloudy.
8	53	7 0	56	64	30,06	73		E	1	Cloudy.
	61	2 0	58	64	29,93	68		E	1	Rain.
9	55	7 0	57	63	29,76	73	0,310	S	1	Cloudy.
	63	2 0	59	64	29,68	74		NE	1	Rain.
10	52	7 0	56	64	29,93	70	0,625	SW	1	Fine.
	70	2 0	69	64	29,94	48		SSW	2	Cloudy.
11	55	7 0	58	64	29,90	67		SSW	2	Fine.
	70	2 0	70	64	29,83	48		SSW	1	Cloudy.
12	50	7 0	54	63	29,92	70	0,072	SW	1	Fine.
	68	2 0	68	64	29,94	54		SW	1	Cloudy.
13	49	7 0	52	63	30,05	68		SW	1	Fine.
	71	2 0	70	64	30,04	45		S	1	Fine.
14	51	7 0	55	64	30,00	61		E	1	Hazy.
	71	2 0	70	64	29,85	51		SE	2	Cloudy.
15	60	7 0	64	65	29,41	60		S	2	Fine.
	68	2 0	66	65	29,50	52		SSW	2	Cloudy.
16	52	7 0	55	64	29,68	63		SW	1	Cloudy.
	67	2 0	67	64	29,74	51		S	1	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Aug. 17	55	7 0	56	63	29,26	73	0,133	ESE	2	Rain.
	62	2 0	60	65	29,45	50		S	2	Fair.
18	52	7 0	55	63	29,51	64	0,037	S	2	Fair.
	65	2 0	65	64	29,50	55		S	2	Fair.
19	54	7 0	55	62	29,58	64	0,044	SW	2	Cloudy.
	66	2 0	66	63	29,62	49		SW	2	Fair.
20	48	7 0	53	62	29,71	63	0,042	SW	1	Fine.
	69	2 0	68	63	29,73	47		W	1	Fair.
21	49	7 0	53	62	29,91	62		W	1	Hazy.
	67	2 0	67	64	30,01	45		NW	1	Hazy.
22	54	7 0	58	63	30,00	66		S	2	Cloudy.
	64	2 0	64	63	29,96	64		S	2	Cloudy.
23	53	7 0	55	63	30,01	69	0,062	SW	2	Fair.
	68	2 0	68	63	30,00	47		SW	1	Cloudy.
24	55	7 0	55	62	29,97	71	0,103	SW	1	Cloudy.
	67	2 0	67	64	29,96	46		W	1	Fine.
25	47	7 0	51	63	30,07	64		W	1	Fine.
	66	2 0	66	63	30,12	50		NW	1	Fair.
26	49	7 0	52	62	30,08	64		SW	1	Cloudy.
	66	2 0	65	63	30,12	50		S	1	Fair.
27	56	7 0	58	62	29,93	65	0,086	S	2	Cloudy.
	66	2 0	65	63	29,85	66		S	2	Cloudy.
28	56	7 0	58	63	29,78	69		S	1	Cloudy.
	68	2 0	68	63	29,74	55		S	2	Cloudy.
29	50	7 0	53	62	29,95	71	0,120	SW	1	Fine.
	67	2 0	67	63	30,05	47		WSW	1	Fair.
30	50	7 0	55	62	30,05	63		S	1	Cloudy.
	65	2 0	63	63	29,97	55		SW	2	Cloudy.
31	56	7 0	57	62	29,68	66		SW	2	Cloudy.
	64	2 0	64	63	29,68	53		WSW	2	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Sept. 1	50	7	0	52	62	30,04	53		WNW	1	Fine.
	63	2	0	63	63	30,17	46		W	1	Fine.
2	47	7	0	51	61	30,30	63		SW	1	Fine.
	66	2	0	66	62	30,32	45		SW	1	Fair.
3	46	7	0	50	61	30,32	64		SW	1	Fine.
	67	2	0	66	63	30,27	50		SW	1	Fine.
4	47	7	0	49	62	30,32	68		SW	1	Fine.
	68	2	0	68	65	30,30	48		SSW	1	Fine.
5	50	7	0	54	63	30,35	65		NE	1	Hazy.
	72	2	0	71	65	30,35	57		E	1	Fine.
6	56	7	0	59	64	30,40	73		E	1	Cloudy.
	66	2	0	66	67	30,38	58		E	2	Fine.
7	52	7	0	56	65	30,32	71		E	1	Fine.
	63	2	0	63	67	30,27	53		E	1	Fine.
8	49	7	0	52	65	30,20	66		E	1	Fine.
	63	2	0	63	65	30,14	48		E	2	Fine.
9	47	7	0	50	65	30,00	71		ENE	1	Hazy.
	69	2	0	69	66	29,98	47		ENE	1	Hazy.
10	46	7	0	48	64	30,07	67		SE	1	Hazy.
	65	2	0	64	64	30,06	60		ENE	1	Cloudy.
11	50	7	0	53	64	29,98	68		NE	1	Cloudy.
	62	2	0	62	63	29,87	59		NE	1	Cloudy.
12	56	7	0	57	63	29,49	76	0,115	E	1	Rain.
	64	2	0	64	64	29,45	70		E	1	Cloudy.
13	55	7	0	55	63	29,64	72	0,690	W	1	Cloudy.
	63	2	0	62	64	29,66	56		WNW	1	Cloudy.
14	48	7	0	53	62	29,62	73	0,050	W	1	Cloudy.
	61	2	0	61	63	29,71	61		N	1	Cloudy.
15	48	7	0	50	62	29,89	70		W	1	Cloudy.
	61	2	0	61	62	29,87	69		SW	1	Cloudy.
16	46	7	0	49	60	29,89	67	0,093	W	1	Cloudy.
	61	2	0	61	62	29,90	53		SW	1	Fair.

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1799	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Sep. 17	55	7	0	56	61	29,78	73		E	1	Cloudy.
	62	2	0	62	62	29,65	69		SE	2	Rain.
18	51	7	0	51	61	29,64	75	0,260	SW	1	Fine.
	65	2	0	65	62	29,66	59		SW	2	Fair.
19	55	7	0	55	62	29,36	69	0,128	SSW	2	Fine. [Much wind last night.
	63	2	0	62	62	29,28	57		S	3	Cloudy.
20	51	7	0	53	61	29,35	70	0,055	SW	1	Cloudy.
	60	2	0	59	61	29,38	66		SW	2	Rain.
21	51	7	0	52	60	29,65	72	0,046	SW	1	Cloudy.
	63	2	0	62	62	29,64	55		S	1	Fair.
22	54	7	0	55	61	29,12	78	0,088	E	1	Rain.
	65	2	0	65	62	29,04	56		SSE	3	Cloudy.
23	55	7	0	55	61	29,20	74	0,248	S	2	Cloudy.
	60	2	0	57	62	29,50	63		N	2	Cloudy.
24	45	7	0	49	60	29,82	60		S	1	Cloudy.
	61	2	0	61	60	29,84	57		SW	1	Cloudy.
25	52	7	0	54	60	29,98	71		SW	1	Cloudy.
	64	2	0	64	62	29,99	56		SSW	1	Fair.
26	53	7	0	54	60	29,82	64		E	1	Fair.
	62	2	0	62	61	29,61	68		SE	1	Rain.
27	52	7	0	52	60	29,53	73	0,210	SSW	2	Cloudy. [Much wind last night.
	60	2	0	59	62	29,75	52		SW	2	Fair.
28	49	7	0	52	60	29,58	70		E	1	Rain.
	52	2	0	51	60	29,13	83		NE	1	Rain.
29	44	7	0	46	60	29,46	74	0,478	SE	1	Fair.
	53	2	0	50	62	29,13	78		SE	1	Rain.
30	46	7	0	48	60	29,32	70	0,363	NW	1	Fair.
	57	2	0	57	63	29,43	57		WNW	1	Fair.

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1799	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Oct. 1	46	7	0	47	61	29,50	72	0,080	S	1	Fair.
	55	2	0	55	62	29,56	54		N	1	Fair.
2	39	7	0	40	60	29,78	69		SW	1	Fine.
	57	2	0	57	62	29,78	57		SW	1	Fair.
3	48	7	0	48	60	29,78	68		SW	1	Fair.
	60	2	0	60	62	29,72	53		SW	1	Cloudy.
4	46	7	0	47	60	29,50	56	0,122	SW	2	Fair.
	56	2	0	54	61	29,50	56		SW	2	Fair.
5	44	7	0	44	60	29,92	65	0,093	SW	1	Fair.
	56	2	0	56	60	30,00	56		W	1	Cloudy.
6	51	7	0	53	60	29,86	83	0,173	SSW	1	Rain.
	60	2	0	60	62	29,90	75		SW	1	Cloudy.
7	56	7	0	56	62	29,91	74		W	2	Cloudy.
	63	2	0	63	63	29,84	73		SSW	1	Cloudy.
8	57	7	0	57	62	29,48	78	0,128	S	2	Cloudy.
	60	2	0	59	63	29,40	62		SSW	2	Fair.
9	49	7	0	49	61	29,54	66	0,060	W	1	Cloudy.
	55	2	0	55	63	29,72	58		WNW	1	Cloudy.
10	43	7	0	43	60	29,94	67	0,028	W	1	Fine.
	55	2	0	55	62	29,98	53		SW	1	Fair.
11	45	7	0	45	60	30,08	73		SSW	1	Fine.
	58	2	0	58	63	30,11	57		SSW	1	Cloudy.
12	48	7	0	49	60	29,88	71		E	1	Cloudy.
	60	2	0	59	62	29,72	75		SE	1	Rain.
13	50	7	0	50	61	29,70	75	0,332	S	1	Cloudy.
	57	2	0	57	63	29,73	61		S	1	Fair.
14	43	7	0	43	61	29,71	71	0,060	SSW	1	Fine.
	57	2	0	54	62	29,84	54		W	1	Fair.
15	41	7	0	42	60	30,02	67		S	1	Fine.
	55	2	0	55	62	30,00	58		S	1	Fair.
16	45	7	0	45	60	29,98	73		S	1	Foggy.
	54	2	0	54	61	29,95	60		SW	1	Cloudy.

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		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Oct. 17	44	7	0	44	59	29,90	71	0,153	W	1	Rain.
	50	2	0	47	60	29,89	67		N	1	Rain.
18	44	7	0	44	58	29,89	73	0,028	N	1	Cloudy.
	52	2	0	52	62	29,83	59		S	1	Fair.
19	48	7	0	49	58	29,65	76	0,135	E	2	Rain.
	50	2	0	50	60	29,68	70		SE	1	Rain.
20	44	7	0	45	58	29,77	74	0,268	NE	1	Cloudy.
	50	2	0	50	59	29,82	67		S	1	Cloudy.
21	43	7	0	46	57	29,55	74	0,098	E	2	Rain.
	49	2	0	49	59	29,49	68		W	2	Cloudy.
22	40	7	0	46	57	29,64	75	0,286	S	1	Rain.
	53	2	0	52	62	29,62	70		S	1	Cloudy.
23	47	7	0	47	57	29,65	75	0,125	SE	1	Rain.
	54	2	0	54	60	29,64	67		SE	1	Cloudy.
24	44	7	0	45	58	29,44	73		NE	2	Cloudy.
	49	2	0	48	59	29,38	73		NE	2	Cloudy.
25	38	7	0	38	57	29,67	71		W	1	Fine.
	50	2	0	50	58	29,74	66		N	1	Cloudy.
26	40	7	0	40	56	30,10	70		N	1	Fair.
	49	2	0	49	58	30,23	61		NE	1	Fair.
27	35	7	0	35	55	30,37	72		E	1	Foggy.
	44	2	0	44	57	30,33	62		ESE	1	Fine.
28	38	7	0	39	55	30,31	70		SW	1	Hazy.
	52	2	0	52	57	30,30	70		SW	1	Cloudy.
29	47	7	0	47	56	30,22	77		SE	1	Cloudy.
	53	2	0	51	59	30,08	60		SE	1	Fine.
30	43	7	0	45	56	29,71	74		E	1	Cloudy.
	55	2	0	55	58	29,60	75		SE	1	Cloudy.
31	49	7	0	49	57	29,46	88	0,110	SSE	1	Fair.
	57	2	0	56	59	29,34	70		SE	1	Cloudy.

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		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Nov. 1	52	7	0	53	58	28,86	79	0,145	S	2	Cloudy [Much wind last night.
	58	2	0	58	59	28,82	62		SE	2	Cloudy.
2	49	7	0	49	59	29,20	76		SW	1	Cloudy.
	55	2	0	53	60	29,48	64		W	1	Cloudy.
3	44	7	0	52	58	29,73	80	0,025	S	2	Rain.
	57	2	0	55	60	29,66	80		S	2	Cloudy.
4	44	7	0	44	58	29,64	71	0,048	S	1	Fine.
	54	2	0	50	59	29,49	71		S	1	Rain.
5	38	7	0	38	57	29,97	69	0,182	S	2	Cloudy.
	48	2	0	48	58	29,71	74		SSW	1	Rain.
6	48	7	0	50	57	29,37	79	0,326	S	2	Rain.
	55	2	0	53	60	29,32	61		WSW	2	Cloudy.
7	40	7	0	40	56	29,29	73		SW	2	Fair.
	49	2	0	49	58	29,39	55		W	2	Cloudy.
8	37	7	0	38	56	29,37	70	0,020	SSW	1	Rain.
	46	2	0	46	57	29,46	60		WNW	1	Cloudy.
9	35	7	0	35	55	29,73	69	0,042	SW	1	Fair.
	44	2	0	44	58	29,71	64		W	1	Fair.
10	32	7	0	32	54	29,97	70	0,045	WSW	1	Fine.
	49	2	0	43	56	29,99	63		SSW	1	Cloudy.
11	41	7	0	40	55	29,68	77	0,257	W	1	Rain.
	48	2	0	48	56	29,72	63		W	1	Cloudy.
12	44	7	0	52	55	29,58	87	0,180	SW	1	Cloudy.
	57	2	0	55	57	29,52	81		SSW	2	Cloudy.
13	45	7	0	47	55	29,67	67		SSW	2	Fair.
	54	2	0	54	57	29,64	65		SSW	2	Cloudy.
14	48	7	0	48	55	29,57	72	0,067	S	2	Cloudy.
	51	2	0	51	57	29,67	69		S	2	Cloudy.
15	46	7	0	47	57	29,95	75		E	1	Cloudy.
	50	2	0	50	58	29,93	73		NE	1	Cloudy.
16	45	7	0	45	57	30,02	78	0,250	NE	1	Rain.
	46	2	0	46	58	30,08	73		NE	1	Fair.

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1799	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Nov. 17	35	7	0	38	56	30,28	78		E	1	Foggy.
	44	2	0	44	57	30,31	76		NE	1	Cloudy.
18	37	7	0	41	54	30,29	72		NE	1	Cloudy.
	47	2	0	47	56	30,26	65		E	1	Cloudy.
19	44	7	0	44	55	30,26	68		E	1	Cloudy.
	48	2	0	48	56	30,29	63		E	1	Cloudy.
20	40	7	0	40	55	30,38	74		NE	1	Fine.
	47	2	0	47	58	30,40	66		E	1	Fine.
21	35	7	0	35	55	30,38	74		NE	1	Hazy.
	41	2	0	41	55	30,37	73		NE	1	Foggy.
22	38	7	0	38	54	30,36	78		NE	1	Rain.
	45	2	0	44	56	30,32	73		NE	1	Cloudy.
23	42	7	0	42	54	30,29	73		NE	1	Cloudy.
	43	2	0	43	56	30,26	68		NE	1	Cloudy.
24	37	7	0	38	53	30,08	70		NE	2	Cloudy.
	40	2	0	39	55	29,97	70		E	1	Cloudy.
25	39	7	0	40	53	29,88	73		E	1	Foggy.
	46	2	0	46	54	29,90	76		NE	1	Cloudy.
26	44	7	0	44	54	30,12	80		E	1	Cloudy.
	46	2	0	46	55	30,14	76		NE	1	Cloudy.
27	43	7	0	43	54	30,15	76		E	1	Fair.
	47	2	0	47	57	30,18	68		E	1	Fine.
28	40	7	0	43	54	30,25	73		E	1	Cloudy.
	44	2	0	44	56	30,23	70		E	1	Cloudy.
29	32	7	0	33	53	30,17	76		W	1	Fair.
	40	2	0	40	53	30,07	76		SW	1	Fair.
30	39	7	0	43	53	29,91	83		S	1	Foggy.
	49	2	0	49	56	29,82	80		S	1	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Dec. 1	45	8 0	48	54	29.56	81		S	2	Cloudy.
	50	2 0	50	56	29.43	83		S	1	Rain.
2	43	8 0	45	55	29,19	85	0,215	NE	1	Rain.
	45	2 0	45	56	29,21	81		NE	1	Cloudy.
3	43	8 0	45	55	29.33	81	0,018	NE	1	Cloudy.
	48	2 0	47	57	29.34	73		NE	1	Fair.
4	42	8 0	42	55	29.42	77		NE	1	Cloudy.
	42	2 0	42	57	29.47	73		NE	1	Cloudy.
5	40	8 0	40	55	29,60	77		E	1	Foggy.
	43	2 0	43	56	29.70	77		E	1	Rain.
6	42	8 0	42	55	29.98	78	0,018	E	1	Cloudy.
	45	2 0	44	57	30,04	77		NE	1	Rain.
7	41	8 0	41	55	30,05	78	0,050	NE	1	Rain.
	41	2 0	41	56	29,98	76		NE	1	Rain.
8	37	8 0	37	53	29.80	75	0,048	E	1	Cloudy.
	40	2 0	39	56	29.73	70		E	1	Cloudy.
9	38	8 0	40	54	29.70	75		E	1	Cloudy.
	44	2 0	44	55	29.74	75		E	1	Cloudy.
10	39	8 0	41	53	29.95	78		NE	1	Cloudy.
	43	2 0	43	56	29.99	71		NE	1	Cloudy.
11	39	8 0	39	53	29.99	73		NE	1	Cloudy.
	42	2 0	42	56	29.99	70		NE	1	Cloudy.
12	36	8 0	36	54	30,07	73		NE	1	Cloudy.
	40	2 0	40	55	30,09	70		NE	1	Fair.
13	35	8 0	35	53	30,13	72		NE	1	Cloudy.
	35	2 0	35	54	30,10	70		NE	1	Cloudy.
14	32	8 0	32	52	30,00	70		NE	1	Cloudy.
	36	2 0	36	53	29,95	69		NE	1	Cloudy.
15	34	8 0	36	52	29,86	73		NE	1	Cloudy.
	34	2 0	34	53	29,79	69		NE	1	Cloudy.
16	32	8 0	33	50	29,66	70		E	2	Cloudy.
	35	2 0	35	52	29,65	67		E	2	Cloudy.

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1799	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	o		o	o						
Dec. 17	31	8 0	31	50	29.62	67		NE	1	Cloudy.
	33	2 0	33	52	29.59	67		NE	1	Cloudy.
18	31	8 0	33	49	29.58	72		NE	2	Snow.
	35	2 0	35	52	29.65	71		NE	2	Snow.
19	21	8 0	22	48	30.10	70		NE	2	Fair.
	27	2 0	26	49	30.20	68		NE	1	Fair.
20	21	8 0	22	46	30.32	68		NE	1	Snow.
	26	2 0	26	48	30.30	60		NE	2	Cloudy.
21	21	8 0	23	45	30.27	64		NE	2	Cloudy.
	31	2 0	30	47	30.22	64		NE	2	Cloudy.
22	31	8 0	31	44	30.18	63		NE	2	Cloudy.
	33	2 0	32	48	30.15	61		NE	2	Cloudy.
23	24	8 0	24	43	30.12	70		NE	1	Fair.
	31	2 0	31	45	30.16	63		NE	1	Fine.
24	29	8 0	32	43	30.07	65		NE	1	Cloudy.
	33	2 0	33	46	29.98	68		NE	1	Cloudy.
25	28	8 0	28	43	29.94	68		W	1	Cloudy.
	34	2 0	34	46	29.93	68		W	1	Cloudy.
26	31	8 0	34	43	29.91	76		N	1	Snow.
	36	2 0	35	46	29.96	73		NE	1	Fair.
27	27	8 0	27	45	30.16	73		NE	1	Fair.
	32	2 0	32	47	30.28	67		NE	1	Cloudy.
28	21	8 0	21	44	30.34	62		NE	1	Fair.
	26	2 0	26	46	30.28	60		NE	1	Fine.
29	26	8 0	28	44	30.09	75		N	1	Snow.
	32	2 0	32	47	30.15	71		N	1	Fair.
30	30	8 0	30	43	30.22	68		NE	1	Cloudy.
	31	2 0	31	45	30.28	70		NE	1	Cloudy.
31	17	8 0	17	43	30.51	68		E	1	Fair.
	23	2 0	23	44	30.54	64		NE	1	Fine.

1799.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	50	20	35,1	50	23	35,6	55	41	49,0	30,43	29,25	29,98	86	61	79,1	0,949
February	56	18	38,2	56	22	38,7	60	42	51,0	30,26	28,88	29,70	92	57	75,2	2,235
March	56	28	39,3	56	28	39,4	62	49	53,6	30,23	29,31	29,84				0,433
April	59	28	44,1	56	30	44,7	58	47	54,3	30,23	28,75	29,62				1,671
May	70	36	52,4	70	40	53,2	62	54	58,7	30,38	29,33	29,84				1,749
June	77	43	58,1	77	49	59,4	67	58	62,1	30,41	29,18	30,04				0,552
July	77	48	62,3	77	52	63,1	68	62	64,9	30,18	29,22	29,82				2,913
August	73	47	60,4	72	51	61,1	66	62	63,3	30,12	29,26	29,81	78	45	59,8	2,209
September	71	44	56,4	71	46	57,2	67	60	62,1	30,40	29,04	29,82	83	45	63,9	2,824
October	63	35	49,0	63	35	49,7	68	55	59,5	30,37	29,34	29,80	88	53	69,4	2,191
November	58	32	44,7	58	32	45,0	60	53	56,1	30,40	28,82	29,87	87	55	71,9	1,587
December	50	17	34,3	50	17	34,7	57	45	50,4	30,54	29,19	29,93	85	60	71,1	0,349
Whole year			47,9			48,5			57,1			29,84				19,662

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.